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THE UNIVERSITY OF ALBERTA

GENERAL DETERRENCE AND CRIME: AN ANALYSIS

by

DOUGLAS F. COUSINEAU ©

A THESIS
SUBMITTED TO THE FACULTY OF GRADUATE STUDIES AND RESEARCH
IN PARTIAL FULFILMENT OF THE REQUIREMENTS FOR THE DEGREE
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DEPARTMENT OF SOCIOLOGY

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THE UNIVERSITY OF ALBERTA

FACULTY OF GRADUATE STUDIES AND RESEARCH

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies and Research, for acceptance, a thesis entitled General Deterrence: An Analysis, submitted by Douglas F. Cousineau in partial fulfilment of the requirements for the degree of Doctor of Philosphy.

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ABSTRACT

The quintessence of general deterrence arguments is based upon the beliefs that there is an inverse causal relationship between the prevention of crimes and swift, certain, and severe legal responses to criminals. Examination of the literature on the subject, however, reveals a medley of concepts and conflicting research findings.

Adequate conclusions about general deterrence are hindered by the failure to define concepts, the references to unobservable and unmeasurable variables, the lack of attention to basic issues concerning research designs and the appropriateness of statistical techniques for confirming or refuting hypotheses. Students of general deterrence have failed to examine the requirements for ascertaining whether or not legal responses are causally related to crime.

The study of general deterrence is often justified because of the alleged importance of its social policy implications. Our analysis leads to the conclusion that social science has not and cannot either confirm nor refute arguments about general deterrence. The use of legal responses to crime may be more adequately defended in terms of their symbolic effects and their moral justifications.

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CHAPTER ONE

CRIME AND SOCIAL DEFENSES

> The sanctions we apply to each other are not merely vindictive. We intend also that moral and legal codes shall change behaviour. We do this by allowing, or forcing, the actor to experience the consequence of his acts. And if the "natural consequences" are not sufficient to mold behaviour as desired, societies attach consequences (Nettler, 1970:144).

Crime threatens one's person, one's property, and those social institutions which attempt to protect persons and property. The threat of danger and harm to these values, and the subsequent desire to protect them, are probably reasons for concern with crime and its control (Nettler, 1974:2).

THE CONCERN WITH CRIME

Students of criminal behaviour are not in agreement as to whether or not there is a "real" increase in serious crimes in North America. McDonald (1969) presents data for serious crimes in Canada from 1950 to 1966 and reports a slight decline. Similarly, the Ouimet Commission reports that, from 1901 until 1966, Canada's serious crimes declined initially and then remained somewhat constant (1969:25).

In the United States, Ferdinand shows that for the city of Boston, since 1849, serious crimes have declined (1967) and further, that in Salem, since the civil war, arrest rates have dropped drastically (1972). In another analysis of crime rates in American cities, Ferdinand (1970) attributes almost all increases in rates of crime to demographic shifts in the population.

On the other hand, Nettler cites data for serious offenses in Canada and the United States from 1960 to 1969 which show drastic increases in crime (1974:8-9). Similarly, Giffen (1965:60) and Fattah (1973:89-163), reporting from the years 1962 to 1970, find increases in serious crimes in Canada. These discrepancies in reported crime rates may actually be more apparent than "real," with studies reporting increases to be focusing on the last ten years.

Regardless of an actual increase or decrease in real crime, the public believes crime to be increasing and in some cases is acting upon these beliefs. These beliefs have been recorded by polls. In 1967, Americans ranked crime as the second most important national problem (Ehrlich, 1972:259; Nettler, 1974:2-4). In addition, in the year 1968, polls recorded crime as America's single most serious problem (Harris, 1968; Gallup, 1968). Concern with crime has also expressed itself in political campaigns (Bilek, 1972:2). Studies show that the concern for crime results in some publics changing their behaviour patterns, avoiding strangers, taking taxis rather than walking, staying at home, and using watchdogs and locks to secure their homes (Conklin, 1971:374; Nettler, 1974:5). In addition, studies of the fear of crime reveal that responses to crime may be independent of the actual crime rate, and dependent upon regard for the community, feelings of safety for one's person, and the degree of interpersonal trust (Conklin, 1971:384). Our assumption is that the concern with real of apparent crime leads to a concern with its control. Responses to crime in terms of attempts to control criminal behaviour lead to the notion of social defense.

Social Defense

Social defense is an aspect of criminology whose students are concerned primarily with the responses to crime. Investigators of social defense attend to the possible causes of differential responses to crime, to the justifications for these differential reactions, and to the consequences of the different ways of responding to crime.

The use of legal responses in the control of crime is justified, in part, by the consequences that are believed to be achieved. In general, there are at least nine kinds of justifications. Whereas dictionary definitions of these justifications tend to "overlap," our study emphasizes their differences.

Vengeance is the justification of punishment inflicted for harm to oneself or to some intimate or ally. In this case, the tendency is to ask for more punishment than the harm of the original offense.

Restitution is a defense of the use of the law requiring repayment to the offended party. This is the basis for concepts of victim compensation.

Atonement is the warrant for the law to seek penitence, that is, reconciliation of God and man. The offender humbles himself and begs God's pardon. This justification was the basis for the creation of "penitentiaries."

Retribution is the imposition of legal responses so that the offender is made to suffer in equal degree as did the victim. This response concerns "just deserts," but entails a sense of balance.

Incapacitation is the use of legal means to restrain the offender so that he does not continue to harm others. Incapacitation does not entail changing the offender.

Symbolic reaffirmation treats the use of the law as a ceremony which asserts "what we are for and what we are against" (Nettler, 1974:34). In this case, the rituals revivify morals and scapegoat offenders.

Rehabilitation is the ratification of legal intervention in order to correct the offender. Proponents of rehabilitation suggest that the use of rewards or combinations of rewards and punishment will induce legal behaviour.

Specific deterrence justifies legal responses that attempt to correct the offender by punishing him.

General deterrence is promoted as a primary reason for implementing the law. By imposing legal responses on offenders, potential offenders are believed to be prevented from becoming actual offenders.

To date, we have little knowledge of the proportions of a citizenry that believe in these justifications or of the intensity of their conviction. Even those occupations that are involved in criminal justice systems have not been representatively studied. However, evidence from polls of the public and discussions of legal responses in legal and social science literature appear to indicate a conflict over the relative merits of rehabilitation and deterrence.[1] This debate turns on issues of the morality of these legal responses and, frequently, on the supposed consequences of these responses for crime control.

DETERRENCE AND SOCIAL DEFENSE

Recent assessments of rehabilitation techniques as modes of crime control have led to the conclusion that, with minor exceptions, the techniques are not effective in changing the behaviour of offenders (Wilkins, 1969; Martinson, 1974). On the other hand, there have been an increasing number of proponents recommending the use of legal responses to obtain deterrence (Ehrlich, 1973; Tullock, 1974), while antagonists of deterrence have also recently been assertive (Bedau, 1973; Fattah, 1973). In addition, the debate over deterrence has clearly pitted the public and many workers in criminal justice systems, who often believe in deterrence, against social scientists who often do not believe in deterrence.

It would seem that the resolution of this conflict between the relative effectiveness of rehabilitation and deterrence is important for several reasons: (1) it is related to theories of the role of legal responses and the problem of social order, (2) it is important for knowledge per se, and (3) there are crucial implications for the formulation of social policy.

Deterrence and Sociological Theory

The conflict over deterrence has implications for sociologists in several ways. First, the role of the law in creation and maintenance of social order is important. In early sociology, social control and law were "one of the main centres of American sociological interest and . . . inspired considerable literature" (Gurvitch, 1945:267). However, the rise of structural-functionalism shifted attention from the possibility of social control due to coercive legal responses to a belief in the maintenance of social order through socialization. Structural-functionalism emphasises informal social controls such as self-conception, internalization of norms, personality variables, family background and sub-cultural factors (Hall, 1945:350; Gibbs, 1966:1947-159; Ellis, 1971). The influence of structural-functionalism grew to the point where, one sociologist, commenting on contemporary sociology, states that "no major theory emphasizes the influence of reactions to crime on the rate of crime" (Gibbs, 1968a:515). However, the renewal of interest in deterrence, has with few exceptions, paid little attention to theories of deterrence (Logan, 1971a).[2]

Deterrence and Knowledge Per Se

The quest for information and knowledge about human behaviour and events is a major aim of social science. Information consists of a statement of fact--where a fact is a sentence that proclaims something is true, within limits, for which there are observable guarantees (Nettler, 1972b:5). By itself, information does not constitute knowledge until regularities are discerned among the information. Knowledge consists of the rules which reveal or impose uniformities among the facts. Gaining knowledge in the social sciences is usually justified by at least one of four reasons: (1) to confirm or refute theories, (2) to permit "understanding," (3) to provide the basis for predicting, and (4) to determine means to alter situations rationally through intervening social policies.

For some students, knowledge _per se_, or _knowledge to satisfy curiosity_, is sufficient reason for studying social behaviour. However, this vindication of knowledge is often neglected, or worse, denegrated (Nettler, 1970:1-3). In general, social policy implications for knowledge about deterrence are the most crucial concern for students of deterrence.

Deterrence and Social Policy

The study of deterrence has implications for social policy. This concern is historical, and theorists of deterrence have made recommendations about social policy since the time of Beccaria (1764) and Bentham (1811). At one extreme, general deterrence is believed to be the foundation of social order, the "cement that binds society together" (Cooper, 1973:168), while some regard deterrence as simply a means for crime control (Morris and Zimring, 1969:137; Cramton, 1969:42; Zimring and Hawkins, 1973:17).

Contemporary appeals to the policy implications are many. Thus, "a prudential interpretation of the findings is all the more advisable because of certain policy implications" (Gibbs, 1968:530). Similarly Jensen claims that

> . . .the implications of the findings for social policy seem quite clear-cut to some. Through stricter law enforcement, one can change the perception of risk and deter potential criminals, effect respect for the police and possibly even effect respect for the law in general. (1969: 200)

So, too, Tittle asserts that "the results of this study have important practical implications" and "it would seem that proposals to reduce crime by improving law enforcement are reasonable" (1969:423). These concerns about the social policy implications are made problematic by the clash of opinion and research evidence about deterrence.

Conflicting Beliefs About Deterrence

Belief in deterrence is widespread and forms a part of "common knowledge" (Roper, 1938; MacIntyre, 1967; Harris, 1971; Bedau, 1973a). MacIntyre notes that when

> . . .the . . . survey in Washington, D.C. asked what they thought was the most important thing that could be done to cut down crime in their city. . . 60 per cent recommended repressive measures, such as more police, police dogs, stiffer sentences, or cracking down on teenagers. (1967: 14)

Vidmar (1973) surveyed a random sample of 144 adult residents in London, Ontario. He reports that most persons interviewed (78 per cent) favoured the death penalty under some circumstances, and of these nearly half (42 per cent) took that stance because of their belief in its deterrent effect. Deterrence is assumed to be obvious (Van den Haag, 1968:28). It is accepted by persons dealing directly with crime, such as the police, prison guards, district attorneys, and judges (Campion, 1955; California Judiciary Committee, 1957; Lunden, 1960; Cooper, 1973). The belief is popular among legislators and is the basic justification of most criminal justice systems.

The rejection of deterrence perspectives is common among social scientists. Some psychiatrists reject the idea (White, 1933:175; Roche, 1938; Abrahamsen, 1960:246), and at least one asserts that belief in deterrence is itself criminal (Menninger, 1966:108).

Some sociologists consider deterrence to be a myth, akin to belief in the arcane. Barnes and Teeters claim that deterrence "is scientifically and historically on a par with astrological medicine, the belief in witchcraft, or the rejection of biological evolution" (1951:314). In the light of conclusions about deterrence in criminology textbooks, Barnes and Teeters' contention is not unwarranted. Toby found that all but one of eleven textbooks concerned with the "punishment" of criminals concluded that such responses are both ineffective and unscientific (1964:332). Jeffrey reports that "all criminology students learn this as a basic principle" (1965:299). This "basic principle" has been attested to by penologists and criminologists (Schuessler, 1952; Sellin, 1955, 1959, 1967; Johnson, 1964; Chambliss, 1967; Taft and England, 1968; Morris and Zimring, 1969; Quinney, 1970; Bloch and Geis, 1970).

An agnostic position on deterrence exists, but is rare.

Cressey for example, indicates that the support of legal reactions to crime

> . . . is always based on some value which punishment is assumed to have. We do not know that imprisoning men deters others, reinforces anticriminal values, corrects criminals or in some other way promotes social solidarity. However, neither do we know that inflicting pain by imprisonment or some other means is an inefficient system for maintaining, or restoring, social integration. (1966:187)

Social Scientists and Social Policy

The role of the social scientist and his research is not a minor one in legislative issues about the effectiveness of actual or potential legal responses aimed at deterrence. These beliefs are particularly important when legislative bodies call upon social scientists to testify or refer to their own works (Goyer, 1972:60).[3] Sellin's involvement illustrates this point: testifying before the British and Canadian government commissions on execution, he states that "research" shows "no evidence" for the effectiveness of the death penalty as a general deterrent (Sellin, 1950, 1955).[4] According to Bedau, social science "evidence" has served as "the mainstay of the social science argument against capital punishment" (1973a:13).

The relationship between researchers and legislators is occasionally one of mutual influence, since legislative decisions have also precipitated deterrence research. When the Canadian government suspended execution for a five-year trial period beginning in 1967, extending it for another five years in 1972, researchers studied the consequences (Teevan, 1972;

Jayewardene, 1972; Fattah, 1973; Jayewardene, 1974).

The researchers' role in governments' decisions is also illustrated by the recent United States Supreme Court decision on the death penalty (Bedau, 1973a:13). In this case, evidence against a deterrent effect provided by social scientists "carried persuasive effect and provided the basic foundation for the Court's favourable decision" (Bedau, 1973b:5).

Not all of the judges considered the "evidence" to be sufficient or up-to-date, and as a result, the Legal Defense and Educational Fund organized a conference to review the evidence. The conference, attended by criminologists from important American criminology centres, concluded that policy-oriented research on general deterrence was necessary and expressed a willingness to utilize the resources of the centres for such a purpose (Bedau, 1973b:9). The aim is to provide research so that courts and legislatures can make "informed" and "rational" decisions (Bedau, 1973b:12).

Social Science Research and Deterrence

Because of the social policy implications, social science research requires rigorous scrutiny to determine its adequacy in providing information about deterrence. In this sense then our task will be a methodological one. Following Lazarsfeld and Rosenberg, we are guided by their use of the term methodology to imply that "concrete studies are being scrutinized as to the procedures they use, the underlying assumptions they make, the modes of explanation they consider satisfactory" (1955:4). We are also motivated by Zimring and

Hawkins' observation that research designs and "methodological problems are of tremendous importance in acquiring knowledge about deterrence (1973:251).

OVERVIEW OF THE ANALYSIS

Having suggested some of the issues with which this study will be concerned, we will present a brief overview of the treatise. A review of research on deterrence shows not only that the research has been inconclusive, but also that it is likely to remain so. It will be our thesis that tests of hypotheses wherein general deterrence is treated as an observable phenomenon are logically and empirically impossible, and research designs as well as statistical techniques for testing hypotheses about general deterrence as unobservable are inadequate. In addition, there has been no provision for an adequate test of the causal aspect of hypotheses about general deterrence.

Chapter Two outlines some of the meanings of deterrence and Chapter Three reviews some research on the topic. Chapter Four begins an examination of some of the requirements for research on general deterrence. The untestable nature of general deterrence as direct hypotheses is noted and attention is then given to the derivation of testable hypotheses. Finally, attention is paid to the problems of confirming and refuting such hypotheses. Chapter Five discusses the measurement of crime required as related to our subject and Chapter Six considers the legal responses necessary for an examination of general deterrence.

Chapter Seven provides a discussion of causality, the criteria by which it can be attributed, and the criteria appropriate to the study of causality in relation to crime, legal responses, and general deterrence. In order to determine investigators' capacity for attributing causality, Chapter Eight reviews the issues of causality as they apply to research designs and discusses the relevance of these for our topic. In Chapter Nine, these issues are applied to research procedures, that is, the ways in which studies are carried out. Chapter Ten examines the potential of statistical techniques in general for determining causality as well as their applicability to the study of deterrence. Chapter Eleven summarizes the discussions, suggests and defends the argument that responses to crime are "better" justified by appeals to their morality rather than to their supposed general deterrent effects.

This study contends, contrary to general opinion, that more research is not needed on general deterrence. As the treatise unfolds, it appears inescapable that the research is not only presently inconclusive but is likely to remain so. Given this conclusion, the resources available for research could be more usefully directed towards questions which can be resolved empirically. After a review of the perspectives on deterrence and sociological research, our study concludes that arguments about general deterrence are not capable of resolution by empirical investigation. It is realized that such a contention is controversial, and it is not submitted lightly. The present treatise is the delineation, and defense of the reasons supporting the contention that the empirical investigation of general deterrence hypotheses is beyond the scope of social science.

FOOTNOTES CHAPTER ONE

[1] Waelder (1952:378) for example, notes the conflicts of: rehabilitation with individual prevention, rehabilitation with retribution, retribution with general deterrence, individual prevention with rehabilitation, individual prevention with general deterrence, and rehabilitation with general deterrence.

[2] As noted, the study of deterrence has implications for sociological theory (Gibbs, 1966:147-159). Conceptual and empirical analysis of deterrence are related to the debates between the structural functionalists and the conflict theorists (Ellis, 1971). Functionalists argue that the social order is the consequence of consensus. Socialization produces agreement upon a common value system, and produces an integrated social system. Conflict theorists, on the other hand, contend that the social order is maintained through coercion by the powerful. Society consists of divergent and conflicting value systems, and the social order is maintained by the use of legal responses. While these are important issues, my focus will be on the social policy implications.

[3] Goyer, in debating the issue of capital punishment in the Canadian House of Commons in 1967 asserts "Dr. Sellin's studies showed that abolition has no effect on the crime rate" (1972:60).

[4] On April 8th, 1975, Dr. Sellin participated in a Workshop on Capital Punishment in Canada, sponsored by the Solicitor General of Canada. At this conference he reiterated his stance and still argues that his research supports the position that there is no evidence for general deterrence.

CHAPTER TWO

THE MEANING OF DETERRENCE

The word "deterrence" has been used at least since the early sixteenth century.

Originally derived from the Latin verb _deterrere_, meaning "to frighten away or from," the word has not undergone much etymological change.[1] However, the etymological meaning of the word "deter" does not meet the requirements of scientific inquiry. To comprehend the meaning of specific words employed in studies, it is essential to recognize the difference between _concepts_ and _constructs_. _Concepts_ are words that have developed naturally; they are defined by current usage or by the dictionary. _Constructs_ are words which have been purposely invented and are stipulative in the sense that we strike an agreement on their meaning. Constructs, therefore, serve to facilitate scientific inquiry. Although they are not explanations in themselves, they help to develop consistent modes which describe the relationships of things rather than their actual meanings (Nettler, 1970:11).

GENERAL DETERRENCE: THE INITIAL FORMULATION

Initial attempts to formalize the concept deterrence into a construct "general deterrence" were made by Cesare Beccaria (1738-1794) and Jeremy Bentham (1748-1832). Bentham's study, _The Rationale of Punishment_ (1811), is an elaboration of Beccaria's _An Essay on Crime and Punishment_ (1764). Both works argue that the major purpose for the application of criminal law is to achieve *general deterrence*,[2] that is, *to discourage potential offenders from becoming actual offenders* (Monachesi, 1955; Manzoni, 1964; Rothman, 1971:60; Bailey, 1971:9).[3]

For Beccaria and Bentham, general deterrence occurs when legal consequences are attached to criminal acts and awarded to offenders so that others will rationally weigh the advantages of criminal behaviour and choose acts resulting in the least pain. These students of deterrence believe that the legal consequences should result in "just enough" pain to tip decisions in favour of legal behaviour.

Beccaria and Bentham argue that general deterrence is "best" achieved if painful legal responses are *certain* to occur, that is, inevitable when someone commits a crime; if they are *swift*, in that they are quickly imposed and carried out; and if they are *continuous*, or applied frequently and regularly. These philosophers believe that legal responses that are mildly harsh but used frequently are more effective than severely harsh consequences imposed rarely. As a result of their emphasis on continuousness, they protest "extremely harsh" and "cruel" methods of treating criminals, and recommend the abolition of corporal and capital punishment. For Beccaria and Bentham, *severity* is less important than are certainty, swiftness, and continuousness of legal reactions to crime. Further, these writers believe that the judicious application of the law for persons committing minor offenses will produce a generalizing effect, deterring persons from the commission of both the minor and serious crimes.

Beccaria and Bentham believe that general deterrence depends upon the general population knowing the consequences of breaking the law. As a result, they are advocates of public education about legal responses.

To summarize, Beccaria and Bentham attempt to expand the concept of deterrence into the construct of general deterrence. Believing in an informed, rational public, they argue that the certain, swift, and continuous imposition of painful legal consequences upon offenders will discourage potential offenders from becoming actual criminals.

PERSPECTIVES ON DETERRENCE

The use of the term deterrence is complicated by its many meanings. Modern literature focuses on various elements of the initla formulation of general deterrence by Beccaria and Bentham and interprets it in a variety of ways.[4] For example, Bedau refers to the initial formulation as the "classical doctrine" and claims that severity, compulsion through fear, and deterrence of "others," rather than of the offender himself, are the main objectives (1967:261). Referring to the "classical school," Claster also points to fear and threat as its earmarks (1967:50). Gibbs maintains that "the deterrence proposition centres on the anticipation of certain and severe legal consequences" (1968:518). Mabbot states that the classical focus of "the deterrence theory" is the belief in the threat rather than the actual legal responses (1971:41).

Schuessler contends that the fear of consequences is the basis of the "deterrent viewpoint" (1971:182). Teevan claims that the "traditional argument" stresses the deterrence of others from committing crimes similar to those of the offender (1972:153).[5] These examples suggest that the term "deterrence" encompasses a variety of views which render it ambiguous.

In the social science sense, there are no theories of deterrence. The construct has not been developed past its "impressionistic stage" (Wilkins, 1962:326). There have been suggestions that theories might be developed by incorporation of the construct into social exchange and social learning theories (Zimring and Hawkins, 1973:2; Logan, 1971a:34-69; Tittle and Logan, 1973:371).

The Morality of Deterrence

Much of the controversy about deterrence results from differing moral beliefs about whether or not it should be the purpose of legal responses to crime (Hart, 1957:446; Grupp, 1971; Gerber and McAnany, 1972; Bailey and Smith, 1972:530; Zimring and Hawkins, 1973:2). These beliefs concern the possibility of discrimination due to the race or wealth of the offender and the irrevocability of execution if the person is innocent (Wolfgang and Riedel, 1973; Van den Haag, 1968:280). The morality of deterrence is questioned if the requisite legal responses are considered to be "cruel and unusual" and, therefore, in violation of human rights legislation (Long, 1973:216). Even those arguments which refer to the effectiveness of legal responses in reducing crime are ultimately moral. They appeal to consequences--crime reduction--which ought to be preferred.

Legal Responses and Deterrence

> In nature there are neither rewards nor punishment --there are consequences (Ingersoll, 1886, cited by Bartlett, 1955:662).

Despite common assumption, the constituents of a legal response are not obvious. Deterrence literature uses interchangeably such terms as "legal actions," "penalties," and "sanctions." Even when only one term, such as "punishment," is used consistently, it may remain undefined and therefore subject to several interpretations, resulting in inferred meanings, contradictions, and inconsistencies. This confusion has led to a variety of perspectives on legal responses and their importance as agents of deterrence.

For our purposes, a legal response is a consequence for an offense against the law, imposed by the state upon an actual or supposed offender because of his offense. The consequence is imposed and carried out intentionally by persons who are authorities of the legal system in whose jurisdiction the offense occurred.[6]

This definition avoids some of the problems surrounding the meaning of deterrence and legal response. Our definition does not include the purposes of legal responses and thus separates a legal response from the intended ends to which it may be put.[7] Further, our definition does not entail an _a priori_ statement regarding the effectiveness of legal responses in achieving purposes. For example, Bloch and Geis claim a sanction to be a "social condition, situation, or force which has the capacity to constrain human behaviour or to compel it to fall within prescribed forms" (1965:35). In this assertion the sanction is effective by definition.

Our definition is broad so as to enable us to include many views about the assumed deterrent effects of legal responses. Finally, our definition avoids the problematic term "punishment." Perspectives on legal responses and the relationships between the term punishment and the meaning of deterrence are discussed below.

Forms of Legal Responses

Students of deterrence are divided on the issue of which forms or legal responses constitute deterrents. Several authors contend that the threat of legal consequences per se acts as a potent deterrent. This approach focuses on how the threats are perceived or viewed. This perspective is referred to as the law-on-the-books approach (Sutherland, 1925; Schuessler, 1952; Sellin, 1967; Schwartz, 1968; Mabbatt, 1971:41).

In contrast, other authors postulate that the legal consequence must actually be imposed upon offenders before general deterrence effects occur. This perspective is referred to as the law-in-action approach (Pound, 1942; Ball, 1955; Sutherland and Cressey, 1966; Armstrong, 1971; Silver, 1968).

Kinds of Legal Responses

Students defend different kinds of legal responses as the determinants of deterrence.

Inculpation involves accusation and takes the forms of arrests, charges, prosecution, and trials. Some proponents insist that threat of inculpation is the most important variable in attaining deterrence (Walker, 1969:63-68). Others maintain that inculpation must actually be carried out (Sjoquist, 1970; Logan, 1974; Tittle and Rowe, 1974).

Adjudication is the kind of legal response concerning the attribution of guilt or innocence and involves trials up to, and including, acquittal or conviction. This legal consequence, whether threatened or carried out, receives little attention in discussions of deterrence (Barber and Wilson, 1968; Jayewardene, 1972; Zimring and Hawkins, 1973:173).

Legal sanctions are the legal consequences of disposition, that is, the awarding of sanctions to a convicted offender, and the legal consequences of implementation, that is, the actual carrying out of the sentence. The threat of disposition and implementation is discussed in much of the literature, especially that on capital punishment (Sellin, 1967; Bedau, 1967). Imprisonment, as an implemented legal sanction, is commonly discussed (Gibbs, 1968a; Tittle, 1969; Logan, 1973).

Types of Legal Responses

Regardless of whether a legal response is discussed as a threat or as an imposition as inculpation, adjudication, or legal sanction, there is consensus that publicly supported responses are more potent deterrents than those lacking such support (Sutherland and Cressey, 1966:11; Chambliss, 1967).

Dimensions of Legal Responses

The dimensions of legal responses believed important by Beccaria (1764) and Bentham (1843) are certainty, swiftness, and continuousness. The relative importance of each of these three dimensions adds another aspect to the confusion surrounding deterrence.

Whereas the classical formulation holds severity to be the least important dimension, contemporary views emphasize it. Andenaes suggests that legal responses, especially punishments, are synonymous with severity (1952:176), and Bailey and Smith correctly maintain that severity is the most frequently discussed and researched dimension of deterrence (1972:531).

Legal students, on the other hand, tend to emphasize certainty (Cramton, 1969:427), but with little agreement concerning what response is to be certain. Some argue for the certainty of sanctions (Packer, 1968:103), others for the certainty of inculpation (McGrath, 1965:8). Still others recommend combinations (Gibbs, 1968a:588). The dimensions of swiftness and continuousness have been neglected by contemporary students.

The literature on deterrence lacks coherence because of the several perspectives on legal responses and deterrent effects. A further source of confusion derives from the ubiquitousness of the term punishment.

Legal Responses and Punishment

A commonly used term in discussions of deterrence is "punishment," but the meaning of this word is not clear. Some authors fail to define the term punishment altogether. For example, Toby (1964) considers at length the question of whether or not punishment is necessary for preventing criminal behaviour, but he does not provide a definition of this term. Sykes (1967), in an entire chapter, elaborately attempts to specify the conditions under which punishment may result in deterrence, without defining either punishment or deterrence.

In other cases, the term punishment has been defined in several ways, so that its meaning is ambiguous. Thus, Bloch and Geis variously view punishment as any interference with the liberty of an individual, the infliction of an injury upon an individual, a technique to coerce acceptable behaviour, and the subjective perception of the intention of a program of social intervention (1965:497-499).

Inculpation, Adjudication, and Punishment

Inculpation and adjudication in themselves are not supposed to involve any legal intentions of punishment. Yet it has been claimed that the "unpleasantness" associated with arrest and trial may constitute a greater deterrent than the court sentence (Zimring and Hawkins, 1973:173). As a result, some students suggest that the term punishment should be expanded to include arrest and trial (Chiricos and Waldo, 1970:215; Logan, 1974; Rowe and Tittle, 1974).

Legal Sanctions and Punishment

The term legal sanction is often defined in terms of punishment.[8] For example, Hart asserts that a sanction must be administered intentionally and must involve consequences considered to be painful (1969:5). Further, Sutherland and Cressey suggest that there are two essential ideas involved in the notion of "punishment" as a legal sanction:

> a) It is inflicted by the group in its corporate capacity upon one who is regarded as a member of the same group.
> b) It involves pain or suffering produced by design and justified by some value that the suffering is assumed to have. (1966:308)

General Deterrence and Punishment

Whether the term punishment is confined to discussions of legal sanctioning, or whether it refers to inculpation and adjudication, the meaning of punishment itself creates difficulties in definitions of legal responses. The concept of punishment may even prove to be unrelated to the study of general deterrence.

Several minor problems in using the term punishment relate to the possible lack of correspondence between the amount of suffering intended and the amount actually experienced by the offender. Petrie (1967) shows that there are at least three kinds of response to pain. Some persons are "reducers" and minimize their pain; others are "augmentors" who tend to maximize their pain; some are "moderates" who are "stimulus-governed" and who neither minimize nor maximize their pain. Petrie suggests an interesting hypothesis to the effect that juvenile delinquents, compared with non-delinquents, tend to be "reducers," and may therefore be less amenable to modification through pain.

Another possible differential in the amounts of suffering experienced and those intended flows from the discovery that legal sanctions intended to be painful may be shorter than those not so intended. For example, there is some evidence that "punitively oriented" judges and magistrates impose *shorter* prison sentences than do "treatment oriented" judges and magistrates (Wheeler, 1968; Wheeler *et al.*, 1968; Hogarth, 1971). In addition, the amount of suffering *experienced* versus the amount *intended* is difficult to ascertain when legal responses are awarded for more than one purpose, such as when they are intended to rehabilitate and punish the offender (Zimring and Hawkins, 1973:38). Further, the offender may interpret the intentions of his captors in a way which differs from their expectations. In regards to "treatment," Lewis makes this point eloquently:

> Let us not be deceived by a name. To be taken without consent from my home and friends; to lose my liberty; to undergo all those assaults on my personality which modern psychotherapy knows how to deliver; to be remade after some pattern of "normality" hatched in a Viennese laboratory to which I never expressed allegience; to know that this process will never end until either my captors have succeeded or I have grown wise enough to cheat them with apparent success--who cares whether this is called Punishment or not? That it includes most of the elements for which any punishment is feared --shame, exile, bondage, and years eaten by the locust--is obvious. (1962:501)

It may be impossible to assess the individual variations to vulnerability to suffering from a legal response. Small increments or decrements in legal responses allow for the greatest play of individual differences, but quantum leaps in the amounts of legal sanctions could reduce the distortion upon possible deterrent effects produced by individual differences. This requires a method of measuring legal responses and will be discussed later in this study.

Punishment Disregarded

The major problem with the concept of punishment is that it may not be relevant in the context of general deterrence. While general deterrence perspectives always refer to punitive responses as requirements for effectiveness, and while legal responses may reasonably be considered "intended" to be punitive, punishment refers to an individual's experience of a legal response, while the construct of general deterrence refers to how these responses, experienced by some, affect others. These two notions are qualitatively different.

Subjective Experiences and Deterrence

While legal responses are supposedly punitive to those who experience them, general deterrence concerns the ways these responses are supposedly experienced by potential offenders. Students of general deterrence have treated the alleged subjective experiences of potential offenders in a variety of ways which can be grouped into two categories--reactions to the legal responses and reactions to the offensive behaviour.

Reactions to Legal Responses

Fear, generating restraint in the potential offender, is the reaction commonly referred to. It is clearly the fundamental concept in the arguments of Beccaria and Bentham, and this emphasis persists in modern literature (Vold, 1958; Coddington, 1971:343).

Fear may inhibit behaviour however, without altering criminal inclination or instilling abhorrence of such behaviour. Nietzche (1887) illustrates this point, noting that

> . . . the broad effects which can be obtained by punishment in man and beast are the increase in fear, the sharpening of the sense of cunning, the mastery of the desires: so that it is that punishment tames man, but does not make him "better." (Bartlett, 1955:727)

Contemporary students describe several possible "emotional" agents of deterrence, some of which are related to fear, such as an "apprehensiveness component" (Matza, 1964:168) or "intimidation" (Hawkins, 1971:163). Broader concepts of internal processes include "controls" (Tappan, 1960:247), "motivations" (Parker, 1968:42), "felt general response" (Van den Haag, 1967:68), and "reinforcement" (Gibbs, 1968:518).

Internal processes are believed to operate at several levels of awareness, such as the conscious level (Tappan, 1960:247), the preconscious level (Van den Haag, 1967:47), and the unconscious level (Parker, 1968:42). Several students, who focus on the conscious level (Calster, 1967; Henshel and Carey, 1972), believe that an awareness of what might happen results in deterrence. Others suggest that deterrence occurs when awareness becomes an expectation of what will happen (Jayewardene, 1973:5). Some authors specify that this awareness must be based upon accurate knowledge about legal reactions (Beccaria, 1809; Bentham, 1843), while others maintain that accuracy is rare and unnecessary (California Assembly, 1968;[9] Mabbot, 1971:41; Henshel, 1972).[10]

A few students of deterrence state that awareness involves recognition of the similarity between oneself and the actual offender, suggesting that such identification facilitates the deterrent effect (Nettler, 1974:33).[11]

Reactions to Offensive Behaviour

Some authors explain deterrence by emphasizing a direct reaction to legal responses. Others believe that legal responses deter by augmenting the internalization of reactions against behaviour defined as offensive (Sutherland and Cressey, 1966: 11; Osborne, 1968:157). In this sense, legal response generates moral and "socio-pedagogical influences" (Andenaes, 1966). Hawkins refers to this process as "the educative-moralizing function of the law," "the moral or socio-pedagogical influence of punishment," and "the educative and habituative effects of our penal sanctions" (1971:163). This approach emphasizes the part that legal responses play in facilitating the belief that disobedience is wrong and offensive in itself.

Degrees of Deterrent Effects

Deterrence has been discussed in terms of the extent of its occurrence. Total deterrence refers to deterrence as a dichotomous effect, which either does or does not occur (Van den Haag, 1967:280). Partial deterrence refers to incremental effects, so that deterrence is described in terms of "more or less" (Jensen, 1969). Absolute deterrence concerns the potential of a specific legal response to deter, while marginal deterrence concerns the relative effectiveness of alternative legal responses (Zimring and Hawkins, 1973:14).

Deterrence may also be considered as achieved either directly or indirectly. Direct effects are thus held to occur when changes in legal responses alter the rates of criminality, independently of any other changes in the community. Indirect effects supposedly occur when changes in the legal responses alter the "normative climate" of a community, which in turn alters the rates of criminality (Bowers and Salem, 1972:428).

Deterrent effects have also been conceived as primary and secondary. Primary effects are "offense-specific," that is, legal responses to a specific crime act as deterrents only for that kind of crime. Secondary effects are "offense-general," that is, the legal responses to a specific crime affect the rates of similar offenses. The former is the more pervasive view of deterrence (Appel and Patterson, 1965:450), while the latter is the initial stance.

Who is Deterred?

The question of who is deterred by legal responses is not obvious. The most frequent arguments refer to the entire society as being deterred by legal responses. This position entails the belief that all members of a society have a predilection for criminal involvement and that all "need" to be deterred (Blackstone, 1768:4; Wechsler and Michael, 1973:731; Ball, 1955:399). Other perspectives limit the range of the deterred to those living in the community or the areas where the laws are enforced (Morris, 1966:631; Mundle, 1971:59). Yet, other views assume that the majority of the population abhors criminal acts and does not require deterrence (Dession, 1962:5; Osborne, 1968:157). Some authors narrow the scope of deterrence to very likely offenders (Ball, 1955:351; Wilkins, 1962:324; Wootton, 1963:97). It is general accepted, however, that some groups, such as children, the retarded, the insane, and persons who are attracted by legal repercussions are not deterrable (Andenaes, 1966:10; Zimring and Hawkins, 1973:98).

General and Specific Deterrence

One of the major problems interfering with the evaluation of the effects of legal responses on crime results from the failure to consistently differentiate general from specific deterrence (Cousineau, 1973:153). Specific deterrence refers to the degree to which legal responses are effective in changing the behaviour of a particular offender who is subjected to them. General deterrence refers to the degree to which the legal responses for criminals affect the behaviour of potential offenders.

The distinction between general and specific deterrence conveys the same idea as does the distinction between general deterrence and simple deterrence by Andenaes (1971a:537); the distinction between individual and general prevention by Sellin (1967b:247); the distinction between general and special deterrence by Cramton (1969:424); the distinction between general and special deterrence by Morris and Zimring (1969:139); the distinction between general and special effects by Zimring (1971:2); the distinction between general preventative activity and individual preventative activity by Sellin and Wolfgang

(1964:2); the distinction between social and individual deterrence by Teevan (1972:154); and the distinction between general and special deterrence by Nettler (1974:259). It is also implied by Bloch and Geis when they state that deterrence refers to "the effect of a penalty in keeping both the individual who offended and others who might do so from behaving in such a manner" (1965:495).

Recently, Zimring and Hawkins have argued that the distinction between specific and general deterrence is a specious one because both are achieved by an identical process (1973:72-74). This process consists of making individuals "sensitive to future threats because of present punishment" (p. 73). While one cannot debate definitions, this writer believes there are five arguments which make the differentiation between general and specific deterrence a useful one. First, general deterrence is the initial construct. Second, general deterrence is the ubiquitous concern, in part, because it is the focus of debates about the effectiveness of execution for deterrence. Third, it is the most inclusive approach to deterrence, incorporating specific deterrence through the argument that offenders are still potential future offenders. Fourth, the emphasis in the idea of deterrence on the consequences of legal responses for persons who do not "experience" them, as do actual offenders, is qualitatively different from the concept of specific deterrence. Fifth, the Zimring and Hawkins definition begs the question. It moves us from the open question, "does deterrence occur?" to the acceptance of a definition which specifies in a presumptive manner the way in which it occurs. In summary, the importance, ubiquitousness, broadness, and conceptual difference point to the choice of general deterrence as the most representative construct.

The Sociological Perspective on General Deterrence

General deterrence is commonly viewed as a process taking place within individuals. Thus Bedau defines deterrence as

> . . . a given punishment (P) is a deterrence for a given person (A) with respect to a given crime (C) at a given time (T) if and only if A does not commit C at T because he believes he runs some risk of P if he commits C, and A prefers, ceteris paribus, not to suffer P for committing C. (1970a: 206)

However, general deterrence may also be approached as a sociological problem. Nettler advises:

> The public question. . . "What accounts for changes in crime rates and differences in crime rates between populations?". . . is a sociological one. It asks for an explanation of the behaviour of aggregates. It need not receive the same kind of answer, then, as the psychological question that asks "Why did he do it?" (Nettler, 1974:12).

Morris notes that specific deterrence refers to "the microcosm of the group of convicted criminals," whereas general deterrence refers to "the macrocosm of society as a whole" (1966:627). This construct is also implied in Bedau's differentiation between two types of deterrence which he designates "Deterrence One," defined above (Bedau, 1970a:206) and "Deterrence Two," which he defines as follows:

> . . . a given punishment (P) deters a given population (H) from a crime (C) to the detree (D) that the members of H do not commit C because they

> believe that they run the risk of P if they commit C and, ceteris paribus, they prefer not to suffer P for committing C.

What these definitions do is make us aware that general deterrence, as an individual process, can be approached sociologically using crime and legal response rates comprised of aggregates of individuals. As such, general deterrence becomes an attempt to explain some of the changes in crime rates within and between populations.

Having outlined some of the meanings of general deterrence the following chapter summarizes a few of the attempts to research this construct.

FOOTNOTES CHAPTER TWO

[1] The Oxford English Dictionary defines "deter" as "to discourage and turn aside or restrain by fear; to frighten from anything; to restrain or keep back from acting or proceeding by any consideration of danger or trouble."

[2] From this point the terms general deterrence and deterrence will be used interchangeably unless otherwise specified.

[3] In most instances, quotations and references are cited from primary sources. Some exceptions are made where the original work is in a language other than English, and where the original source is very old and the secondary source is considered reliable.

[4] Bedau states that the classic doctrine of deterrence is the view that

> . . . by far the most common way to employ a punishment as a preventive of crime is to adopt a sufficiently severe penalty so as to compel general deterrence out of fear of the consequences of disobedience (1967:216)

Claster notes that the classical school of criminology "asserts that criminal behaviour can be deterred by fear of punishment" (1967:80). Schuessler contends that the deterrent viewpoint consists of the idea that "people are believed to be deterred because they fear punishment" (1952:55). For more comprehensive discussions of the classical school of criminology and the classical theory of deterrence, see Monachesi (1955); Vold (1958); Bloch and Geis (1965); Grupp (1971); Coddington (1971); and Reckless (1973).

[5] Thus Teevan suggests that "the example set by a criminal getting punished will discourage other persons from committing similar crimes" (1972:153, emphasis mine). See also Schwartz and Skolnick (1962:133) and Armstrong (1971:27). The problem here is one of establishing classes of crime which are "similar," once criteria for "similarity" have been established.

[6] This definition paraphrases Hart's definition of a legal sanction (1969:5).

[7] Schwartz and Orleans specify a legal sanction to be "an officially imposed punishment aimed at enforcement of legal obligations (1967:274, emphasis mine). Similarly, Cramton defines a legal sanction as "a preventative technique that involves the official imposition of consequences. . . for

the *purposes* of enforcing legal obligations" (1969:432, emphasis mine), and Cooper defines deterrence as "any measure *designed* actively to impede, discourage, or restrain the way in which another might think or act" (1973:164, emphasis mine). Grupp simply notes that deterrence "is the primary *purpose* of the state's sanctions" (1971:71, emphasis mine).

[8] Gibbs (1966) provides a comprehensive discussion of the general concept of sanction, in which he notes the problems of differential perception involved. Gibbs suggests a differentiation between two components of the term sanction: hedonic components and inducement components. *Hedonic* components are phrased in terms of punishments and rewards, and fail to resolve the dilemma that punishment and rewards may be differentially defined by the administrators and the recipients of sanctions. *Inducement* components are phrased in terms of sanctions which are administered with the intention of the administrator encouraging or discouraging specific behaviour patterns in the person sanctioned. As such, they avoid some of the problematic aspects of the subjective nature of punishment. While the term punishment could be usefully employed to refer only to those sanctions which are intended to inhibit behaviour (whether or not they successfully do so), this usage does not reflect the dominant connotation of the term.

[9] For example, Ball suggests that among the factors which are necessary for the deterrent effect, are "the individual's knowledge of the law as well as the prescribed punishment" (1955:348). Meehl makes a more typical assertion when he simply states that "the general deterrence notion in criminal law presupposes knowledge (or, more precisely, belief)" (1971:74) with regard to sanctions.

[10] Proponents of the symbolic interactionistic perspective to deterrence claim that the objective, "real" responses are not important. It is what the individual thinks the responses are which determines his response (Henschel, 1972; Waldo and Chiricos, 1972). These views may have merit in the sense that the most accurate knowledge of legal responses appears to exist among those who have experienced them. For example, the California Assembly Committee on Criminal Procedure examined the extent to which the public was aware of legislative changes regarding the severity of possible legal sanctions. It reports that

> . . . a range of 21 to 40 per cent of the respondents had complete ignorance or were unable to even guess the maximum sentence for crimes. Furthermore, even among those who made an estimate, the per cent of correct responses ranged from 8 to 39 per cent. If one combined the number of correct responses into a single index score of accuracy, no one person

> correctly answered all 11 questions about penalties, while, at the other extreme, 69 per cent of the respondents answered 3 or less items correctly. (California Assembly Committee on Criminal Procedure, 1968:12)

For further elaboration of these data, see Crowthers (1969: 147-158).

[11] Schwartz and Skolnick suggest that the imposition of a sanction, while intended as a matter of overt policy to deter the public at large, probably varies in its effectiveness as a deterrent, depending upon the extent to which potential offenders perceive themselves as similar to the sanctionee (1964:104). Nettler notes that

> . . . it is . . . assumed, with some good evidence, that you and I will get the message more clearly, the more closely we identify ourselves with the miscreant. It is believed that the more we resemble the punished person, the more forcefully his penalty threatens us and deters us. (1974: 33)

CHAPTER THREE

GENERAL DETERRENCE AND RESEARCH

Despite the importance of the beliefs about general deterrence, it has been neither confirmed nor refuted by research. In fact, research has been so limited in scope and inadequate in method that little progress has been made since the initial assertions by Becarria (1764) and Bentham (1811). This limitation has been acknowledged by The President's Commission on Law Enforcement and the Administration of Justice, which notes that although the criminal justice system presumably works to reduce crime by deterrence, this "method is extremely complex and our knowledge about it is very inadequate at present" (1967d:55). It also states that, until there is a "major research program involving analysis and experimentation," decisions regarding deterrence "will be based on intuition rather than on observed fact."

These statements are made in the face of many attempts to research deterrence. Early studies of deterrence focus on capital punishment for homicide by Dann (1935), Schuessler (1952), Sellin (1957), Savitz (1958), and Sellin (1961, 1965, 1966, 1967). These studies are followed by several attempts to focus on legal responses for other offenses including the works of Butel (1957), Chambliss (1966), Claster (1967), Schwartz and Orleans (1967), Schwartz (1968), Barber and Wilson (1968), Campbell and Ross (1968), Jensen (1969), Chiricos and Waldo (1970), Bailey, Gray, and Martin (1970), Salem and

Bowers (1970), Jayewardene (1972), Bowers and Salem (1972), Fattah (1972), and Teevan (1972).

Research on deterrence also includes three integrated series of studies. One series began in sociology with the study by Gibbs (1968a), whose data were re-analyzed, and re-interpreted by Gray and Martin (1969), Bean and Cushing (1971), and Erickson and Gibbs (1973). A second series in sociology was initiated by Tittle (1969), re-analyzed and re-interpreted by Logan (1972), and Tittle and Row (1974). A third series of studies emerged in the economics literature, consisting of the work of Ehrlich (1972), Votery and Phillips (1972), Phillips and Votery (1972), Carr-Hill and Stern (1973), Ehrlich (1973), Orsagh (1973), and Sjoquist (1974).

In addition, there have been numerous attempts to summarize and evaluate studies of deterrence (Andenaes, 1952; Schuessler, 1952; Ball, 1955; Toby, 1964; Andenaes, 1966; Biddle, 1969; Morris and Zimring, 1969; Osborne, 1969; Bedau, 1971; Bailey and Smith, 1972; Brooker, 1972; Tittle and Logan, 1973; Cousineau, 1973; Bedau, 1973; Erickson and Gibbs, 1973; Tittle and Rowe, 1973; Zimring and Hawkins, 1973; Tullock, 1974; Wellford, 1974; Silver, 1974).

Conclusions by the researchers and reviewers range from the belief that the research "shows" legal responses to be effective as deterrents (Tullock, 1974:110) to the contrary belief that legal responses may actuate crime (Schwartz and Orleans, 1967:276). Between these extremes a spectrum of conclusions attest that there is "some" evidence for deterrence

(Tittle and Logan, 1973:385), that research "shows" legal responses are not effective as deterrents (Sellin, 1967; Bedau, 1967), and that the research is contradictory and inconclusive (Waldo and Chiricos, 1972:522).

Despite this variety of conclusions about deterrence research, there is a common theme in this body of literature, namely, an appeal for more research. This attests to a major omission in the deliberations about deterrence. Focusing on the "findings" of research, there is rarely any emphasis on the substantive conceptual and methodological issues.

Thus Hall's criticism that students of deterrence are biased and that they misuse history and statistics, has been ignored (1945:360). Zimring and Hawkins, contemporary reviewers, indicate that methodological problems are still the major barriers to knowledge about deterrence (1973:251).

The variety of conclusions about deterrence can be explained partially by the fact that most researchers and reviewers use different constructs of deterrence, different measures of crime and legal responses, different sampling units for analyses, and a variety of research designs and statistical techniques (Meehl, 1971; Erickson and Gibbs, 1973; Cousineau, 1973, 1974). For example, conclusions about deterrence have been inferred from a variety of kinds of research and sources of data, such as historical material (Rusche and Kirchheimer, 1939), case histories (Andenaes, 1966:962), experimental data on animals (Church, 1963) and humans (Aronfreed and Biber, 1965), the statistical analysis of cross-sectional data (Gibbs, 1968a; Tittle, 1969), longitudinal data (Chiricos and Waldo, 1970), time-series data (Sellin, 1967; Barber and Wilson, 1968), and interrupted time-series data (Chambliss, 1966; Ross and Campbell, 1968). With few exceptions, the limitations of such data and the implications of such data and the implications for inferences about deterrence have not been critically examined (Waldo and Chiricos, 1972:524; Cousineau, 1973:153; Tittle and Logan, 1973).

SOME STUDIES OF DETERRENCE

Our selected summary of studies of deterrence focuses on those which are sociological, that is, those which examine crime and legal response differences and changes within and between populations. We are aware of several studies of deterrence where individuals are the research focus, and will occasionally make reference to some of them (Claster, 1967; Assembly Committee on Criminal Procedures, 1968; Jensen, 1969; Henshel and Carey, 1972; Waldo and Chiricos, 1972; Teevan and Whitehead, 1972).

Our summaries state the conclusions and findings of the researchers, but all such findings, whether for or against deterrence, should be considered equivocal.

Some Initial Studies

The initial studies of deterrence focus primarily on execution as a possible deterrent for the crime of murder. Emphasis is on the severity of capital punishment, in terms of the legislative provision for it, or the publicity attendant upon the threat of execution. Some selected studies follow.

Publicity and Homicide

Dann studies the supposed impact of publicity about executions on homicide rates (1935). He argues that, if execution is a deterrence, the effect of well-publicized executions should show lower homicide rates in the days immediately following. This effect should be prominent in the community where the executed offender lived, where the offense had taken place, and where the trial and execution were well publicized.

Dann was able to find five cases which met the above requirements. There was one case for each year for the years 1927, 1929, 1930, 1931, and 1932 in the city of Philadelphia. Data on homicide for 60 days preceding and following execution were obtained from the coroner's office and checked with the prosecutor's office in order to eliminate any case which did not result in the offender being accountable for the death penalty.

Dann determines the number of homicides for each of the five cases for the "before" and "after" time periods, which he sums up. He finds a total of 204 homicides, 91 in the time periods preceding execution and 113 cases following the executions. He then groups the homicides into 10-day periods for both the "before" and "after" time periods and compares each 10-day "before" time period with its appropriate 10-day "after" period. Of the total number of homicides, 19 resulted in sentences for murder, 9 in the "before" time period and 10 in the "after" time period. Dann concludes that this shows no evidence for the impact of well-publicized executions on the murder rate (1935).

Savitz (1958) extends Dann's (1935) argument suggesting that the deterrent effect of capital punishment can be determined by assessing the impact of the "maximum publicity of trial, conviction and the sentencing to death" (1958:338). Savitz contends that publicity about the execution itself is often "perfunctory" and appears much later than does the publicity surrounding the trial and sentence of death.

Savitz examines four cases of murder (1948:339). He selected his cases from the newspaper, The Philadelphia Inquirer, where they had received thirty to seventy inches of publicity. Using homicides known to the Philadelphia police, he examined the 8-week period prior to and after each case. He sorted, from all the homicides, those which he classified as "Possible Capital Crimes" (where the offender was probably eligible for execution) and those which were "Definite Capital Crimes" (where the prosecution had proceeded on a charge of first-degree murder)(Savitz, 1958:340).

Savitz analyzed his data for each case for each week prior to and following sentencing. For each case, he computed the number of "Definite Capital Crimes" cases, the number of "Possible Capital Crimes" cases, and the total number of both types of cases. He then added all the "Possible" and "Definite" murders for all four cases together to get the overall totals. These overall totals were then averaged for the "before" and "after" time periods. Savitz interprets his data as showing that there is no pattern to "indicate" deterrence (Savitz, 1958:341).

Capital Punishment and Homicide

Schuessler (1952) uses the vital statistics on homicide, collected by the United States Census Bureau and the Federal Judicial Statistics, for data on executions for the years 1925 to 1949 (1952:183-184). He asserts that, for the U.S. in general, homicide rates increased steadily from 1900 to 1926 and then decreased sharply until 1945, followed by an upward trend to about 1950. Schuessler (1952:184) claims that with some exceptions patterns of homicide rates in individual states are generally the same as the overall national pattern.[1] Schuessler claims that homicide rates showed large regional differences: low rates in New England and high rates in the South (1952:154). He states that these differences in homicide rates exist because murder is a complex sociological event--primarily a function of the season of the year, ecological area, race, and sex.

Schuessler claims that the rate of executions for persons sentenced to death remains constant from 1933 to 1943 (1952: 185). For the time period 1933 to 1939, 80 per cent of those sentenced to death were executed, while in the time period 1940 to 1945, 81 per cent of those sentenced to death were executed. He states that these data contradict the belief that the ratio of executions to persons sentenced to death is declining. Schuessler also contends that, in terms of the numbers and percentages, executions remain proportional to homicide (1952:185).

Schuessler concludes that for the time period 1925 to 1949 execution practices and policy are "fairly stable" and that the changes in and differences among homicide rates "cannot be attributed to changes in the use of the death penalty" (Schuessler, 1952:185).

The analysis of homicide and execution for the United States in general is followed by a comparison of abolition and death penalty states (Schuessler, 1952:186). However, Schuessler classifies states in terms of the legal provision for the death penalty and does not use execution rates as before. He compares homicide rates per one hundred thousand of the population for capital punishment and abolition states for all states for the years 1929, 1933, 1938, 1943, and 1949. He interprets his data as evidence that death penalty states have homicide rates that are two to three times higher than those for abolition states (Schuessler, 1952:186).

Schuessler's third analysis is of states that have been grouped by contiguity, that is, where states share a common border and where at least one of the neighbouring states has the legal provision for capital punishment while the others do not (1952:187). He compares homicide rates for five groupings of such contiquous states for the years 1931 to 1935; 1939 to 1940; and 1941 to 1946. Schuessler states that his data indicate that the presence or absence of capital punishment makes no difference in the homicide rate (1952:186).

European data are the basis for this researcher's fourth analysis. Using statistics from Sellin's (1950) submission to the British Royal Commission on Capital Punishment, collected for the countries of Sweden from 1754 to 1942 and the Netherlands from 1850 to 1927, the Swedish homicide rate per one hundred thousand of the population, and the Netherlands' homicide rate per million of the population, are used to "illustrate the fact that the independence between murder rates and the death penalty is not a peculiarity of American culture" (Schuessler, 1952:188).

The certainty of execution is the focus of Schuessler's fifth analysis (1952:189). He states that the correlation between execution rates and homicide rates for 41 death penalty states between 1937 and 1949 is .48. When the risk of execution measured as the number of "executions-for-murder" per one thousand homicides for the years 1937 to 1949 in the 41 death penalty states, the correlation with homicide rates is a negative, -.26. Schuessler interprets this finding as an "indication" of a slight tendency for the homicide rate to diminish as the probability of execution increases (1952: 190).

In order to determine the consistency of this slightly negative relationship between execution and murder rates, Schuessler grouped the 41 death penalty states into four categories, depending upon their homicide rate (highest, upper-middle, lower-middle, lowest). By determining the average homicide and execution rate for each grouping, he found the ratio of the average execution rate to the average homicide rate.

For the states in the highest, upper-middle, and lower-middle categories, the data are consistent with his report of an overall slightly negative relationship between execution and murder rates. However, because the category of states grouped as "lowest" in homicide rates show an increase in murder rates with an increase in execution rates, he points out that ". . . the homicide rate does not consistently fall as risk of execution increase" (Schuessler, 1952:191; emphasis mine). Thus he concludes that these data are "negative" evidence, refuting deterrence theory (1952:191).

In his final analysis, Schuessler takes 11 death penalty states from the years 1930 to 1949, which had a wide range of execution rates. He argues that, if deterrence arguments are valid, then states with a large number of executions in one year should experience low murder rates the next year. Schuessler argues that, because some of the correlations between the number of executions and subsequent murders were high, and because of the correlations 4 were negative and 7 were positive, that execution and murder rates are independent (1952:191).

Sellin (1961, 1967a, 1967g) attempts to ascertain the effect of the legislative provision for the death penalty on homicide rates. He argues that this effect can be measured by comparing contiguous or neighbouring states where at least one state does not have the death penalty. Sellin then selects 5 groups of 3 adjacent states and compares the homicide rates for the states with capital punishment to those which do not. His 5 sets of states are compared on data for the years 1920 through 1963, based upon homicides reported in the American Vital Statistics. Sellin assumes that, while homicide rates based on Vital Statistics include murder and manslaughter, the ratio of these two offenses remains constant over time and place.

Sellin, like Schuessler (1952), presents his data in a series of charts showing the rates for each state in each of the 5 sets. He does not provide any statistical analysis of the data, but asserts that the rates for each state in each set follow the same general pattern. He concludes that, from the data presented, it would not be possible to detect the states which had the legal provision for capital punishment from those which did not. Sellin concludes that these data are evidence for the ineffectiveness of capital punishment as a deterrent of homicide.

Some Independent Studies

While the initial studies focus primarily on capital punishment and deterrence, other researchers have become interested in the impact of various kinds of legal responses on offenses other than homicide.

Beutel (1957) compares the penalties provided by law for passing bad cheques in American states with the actual number of such cases of passing such cheques. Finding that many who write these cheques are professional criminals, Beutel attempts to investigate the supposed influence of legal provisions on habitual versus occasional offenders.

In Nebraska, enforcement of bad-cheques-laws varies markedly among different counties. The fact that writing bad cheques for over thirty-five dollars is a felony in the state, does not seem to affect the rate of cheque-passing. In Colorado, passing bad cheques is a misdemeanour, while in Vermont it is not a criminal offense at all. Comparing Nebraska, Colorado, and Vermont by the number of bad cheques per one hundred of population, Beutel finds that Vermont has less than half the number of Nebraska, while Colorado has less than half as many as Vermont.

The severity of penalties, Beutel concludes, has no relationship to the number of bad cheques passed. Further, he states that the seriousness of the penalty imposed on a convicted cheque writer is found to have no influence on the amount of money that the cheque is written for. Beutel formulates these "general" laws as a conclusion to this study:

> 1. Laws will very rarely be enforced as they were either written or intended.
> 2. Inflexibility is often the death of a law in an evolving social system.
> 3. Obsolete, unenforced or unenforceable laws, left unamended, may instigate the break-down of law enforcement.
> 4. Severe punishment seems no greater a deterrent than less harsh laws. (1957)

Schwartz and Orleans attempt to predict the effects of different kinds of sanctions. They examine the questions: (1) Does threatening with punishment discourage would-be offenders from breaking the law? (2) If it does, how is this effect achieved? (3) What are the indirect results of such intimidation? (4) How successful is threatening when compared with other types of sanctions? (1967)

The authors choose income tax compliance as the area of inquiry for their study. They choose this topic because payment of taxes is a serious legal demand placed on virtually all American adults. Also, a very large percentage of the population pay their taxes. Consequently, many ethnically, religiously, and socially diverse groups may be studied and compared. As taxes are paid on a yearly basis, sequential observations can be made from a series of tax forms.

Commonplace violations of income tax laws means that those who do not pay may be compared with those who do. It allows for the estimation of the effect of different "motivations" in different taxpayers, as well as the discovery of which sanctions encourage full payment and which sanctions do not. The objectives of Schwartz and Orleans are: (1) to discover the reaction to sanctions involving threats and (2) to compare these results with appeals to taxpayers' consciences.

The methodology involves use of an experimental design. Two experimental and one control group are selected. These groups are judged to be basically the same in social characteristics. Then the two experimental groups are subjected to two different types of sanctions to determine their reactions and compared to those of the control group which was not subjected to any sanction.

Tax information on all groups is supplied by the Internal

Revenue Department. The subjects are matched through residential information from census tracts. Income levels are ten thousand dollars or more to eliminate those whose income is too low to require reporting. Assignment to the experimental and control groups is done at random. There are 92 people in the "control" group, 91 in the "conscience" group, and 89 in the "sanction-treated" group. All groups are asked, one month before their returns are due, about their reactions to tax policies. Each of the groups is asked questions designed to draw out particular reasons for paying taxes. The "sanction-treated" group is asked: "Under what conditions do you think the government should impose a jail sentence for the willful failure to pay taxes or interest?" Other questions emphasize the severity of government sanctions and the likelihood of apprehension. The "conscience" group is asked questions emphasizing the moral reasons for paying taxes, such as: "Is the willful failure to pay taxes or interest an abdication of the duties of a citizen?" Other questions emphasized non-controversial uses of tax funds, a citizen's obligations, and personal integrity. The control group is given the same basic questionnaire but without those questions which are appeals to the "conscience" or "threats of the law."

The content of the questionnaires is analyzed to determine responses, to the question, "What reasons do you think taxpayers might have for reporting all the interest they earn on their tax returns?" Schwartz and Orleans claim that many social variables influence attitudes toward tax payment. However, the overall findings are interpreted by Schwartz and Orleans as suggesting that (1) inclination to pay taxes may be increased through threat of punishment and (2) appeals to the taxpayer's conscience can be more effective than threats (1967).

Some Recent Sociological Studies

Two integrated series of studies have emerged in primarily sociological criminological literature. These studies focus on the "Index Crimes" of homicide, robbery, burglary, larceny, rape and auto theft; using geographic units of analysis, such as the state or province; creating indices of crime and legal responses based upon published secondary statistics; and applying sophisticated statistical techniques for analyzing the data. The researchers in each series, continue to expand and refine arguments about deterrence within each series, while also debating the relative merits of the two streams (Logan, 1971b; Erickson and Gibbs, 1973). Further, some of the issues raised by these two research streams have resulted in some independent studies (Chiricos and Waldo, 1970; Teevan, 1972).

Gibbs (1968a) initiates a new approach to the study of deterrence. He uses the Uniform Crime Reports and the National Prisoner Statistics to compute indices of certainty and severity of legal responses imposed upon offenders. He measures the certainty of legal responses by the ratio of prison admissions divided by the number of crimes known to the police. He uses the median number of months served in prison by homicide offenders as a measure of severity.

Gibbs attempts to determine the impact of the certainty and severity of imprisonment on homicide rates. His certainty index uses data for offenders admitted to prisons for homicide in 1960 divided by the number of homicides known to the police for the years 1959 and 1960. He calculates severity by determining the median number of months served for homicide by offenders in each state prison as of December 31st, 1960.

Computing his indices for forty-eight coterminous states, Gibbs calculates the average annual rate of homicides known to the police per one hundred thousand of the population for the years 1959 to 1961 inclusive. Gibbs analyzes his data in four ways. First, in order to determine the importance of certainty, he compares state homicide rates between those above and those below the median level of certainty. He computes a correlation for these two groups of states and concludes that there is an inverse relationship between certainty and homicide rates.

Second, Gibbs analyzes his data for the importance of severity in the same way, and finds a similar, though weaker, relationship between severity and homicide rates (1968a).

Third, Gibbs compares the homicide rate for states above the median on certainty, and below the median on severity, with those states below the median on certainty, and above the median on severity. The correlation is interpreted by Gibbs as indicating that the homicide rates are not significantly different between the two groupings.

Finally, Gibbs compares states which are above the median in both certainty and severity with states below the median on both certainty and severity. Gibbs states that the over-all pattern of correlations indicates an association, that is, severity and certainty are jointly negatively associated with homicide rates (1968:523-524).

Gibbs' conclusion is reasonable, stating that, because homicide rates may be also affected by non-legal factors which are not controlled for in his study, the findings only "question the common assertion that no evidence exists for a relationship between legal reactions to crime and the crime rate" (1968a:529).

Tittle, like Gibbs, examines the relationship between certainty and severity for all American states from statistics reported in the Uniform Crime Reports and the National Prisoner Statistics (1969:409-423). His measure of certainty is the number of admissions to state prisons from 1959 to 1963, inclusive, divided by the number of crimes known to the police from 1958 to 1962, inclusive. Severity is determined by the mean length of time served by prisoners released from state prisons in 1960. As a second measure of severity, Tittle uses the median sentence for offenders imprisoned in 1960. Tittle points out that this index differs from that of Gibbs (1968a) who uses the median number of "months served by persons actually in prison in 1960 rather than the time served by those who had completed their prison tenure" (1969: 413, footnote number 24).

Tittle determines severity by the ratio of the average annual numbers of crimes known to the police from 1959 to 1963 per one hundred thousand population as of 1960 (1969: 418).

Introducing a series of control indices, Tittle places the states into somewhat homogeneous groupings. He (1) measures urbanization by the proportion of the population living in cities of twenty-five thousand or more; (2) computes the level of education of the population using the median amount of education; (3) measures the age distribution of the population by the proportion of persons between the ages of fifteen and twenty-nine; (4) calculates the sex ratio for those of the population between the ages of fifteen and forty-nine; and (5) obtains a measure of the degree of "modernism" by calculating the proportion of persons in "non-farm occupations" (Tittle, 1969:414). He asserts that these five variables are "important correlates of the amount of deviance" (1969:414).

Tittle computes his measures of certainty, severity, and crime rate for each state for all of the "index offenses." He then analyzes his data for relationships. First, he analyzes his data for all seven offenses together, followed by an analysis of the data for each offense separately (1969: 415-419).

Tittle interprets his findings as showing that the relationships amongst the variables are "complex" and not straightforward (1969:422). In general, he argues that the findings for certainty and all seven offenses together reveal a negative association. This pattern is also discovered for the analysis of the offenses separately, but with a wide range of associations depending upon the particular offense.

Tittle, concluding that there is a positive relationship between severity and all seven offenses analyzed either together or separately, claims that "it appears that the greater the severity of punishment, the greater the crime rate is likely to be" (1969:416). Discussing one important exception to this conclusion, Tittle finds a significant negative association between severity and homicide (1969:417).

The studies of Gibbs (1968a) and Tittle (1969) are similar in several ways. They use similar indices, the same data sources, and about the same time periods. Both studies claim evidence for a negative relationship between certainty and severity, and homicide.

On one point, however, the two studies are contradictory. Gibbs (1968a) reports a significant negative relationship between certainty and homicide rates and a weak but negative association for severity and homicide rates. Tittle's (1969) data are interpreted as showing just the reverse of Gibbs' (1968a) findings. Tittle finds no evidence for an association between certainty and homicide, but a significant relationship between severity and homicide rates. These contradictory findings are the justification for the research of Chiricos and Waldo (1971:202).

Chiricos and Waldo (1970) suggest that potential offenders are probably more aware of both sudden changes and general trends of the certainty and severity of legal responses than of the specific current levels (1970:203). As a consequence their research attempts to determine the relationships between changes in the levels of severity and certainty, and subsequent changes in the crime rates (1970:202). From the Uniform Crime Reports and the National Prisoner Statistics, Chiricos and Waldo compute three indices of certainty, one for each of the years 1950, 1960 and 1963. Their certainty index consists of the admissions to state prisons for each of these years, divided by the average number of crimes known to the police for that year and the previous year. These indices provide measures for the specific years plus the changes between the three time periods. The researchers measure the changes in levels of certainty among the years 1950, 1960, and 1963 by calculating the percentage differences. For example, the percentage change in the level of certainty between the years 1950 and 1960 is calculated by subtracting the level of certainty for 1950 from the equivalent measure for 1960, and dividing this by the 1950 measure of certainty.

Chiricos and Waldo gauge the severity of imprisonment by computing the median length of time served by inmates released. They compute these measures from the National Prisoner Statistics for the years 1960 and 1964 (1970:204). To assess the percentage change in severity between the two time periods, the authors subtract the measure of severity for 1960 from that for 1964, and divide by the severity measure for 1960. Chiricos and Waldo compute crimes rates, in terms of the number of offenses known to the police per one hundred thousand of the population. They compute four average crime rates for the years 1950, 1951, and 1952; 1960, 1961, and 1962; 1963, 1964, and 1965; and for 1964, 1965, and 1966.

The researchers calculate all their indices of severity and certainty and their crime rates for six of the "Crime Index" offenses, eliminating the seventh index offense category of "sex offenses" because of differences in definition and kinds of offenses between the Uniform Crime Reports and the National Prisoner Statistics. All these indices are computed for forty-eight conterminous states in the United States.

Chiricos and Waldo (1970:206-207) analyze their data in three ways. To test for an association between certainty and crime rates, they compare (1) the measure of certainty for 1950 with the average crime rates for 1950, 1951, and 1952; (2) the measure of association for the certainty level in 1960 with the average crime rates for 1960, 1961, and 1962; and (3) the 1963 level of certainty with the average crime rates for the years 1963, 1964, and 1965. The six offenses are reported as showing no negative relationships between levels of certainty and subsequent crime rates (1970:207).

The authors analyze severity by comparing the 1960 measure with the average crime rate for 1960, 1961, and 1962; and by comparing the severity measure for 1964 with the average crime rate for 1964, 1965, and 1966.

Measures of association for the six offenses are interpreted as revealing no relationship between the severity index and subsequent crime rates (Chiricos and Waldo, 1970:208).

Chiricos and Waldo then compute the percentage of change in certainty and the percentage change in subsequent crime rates for each of the six offenses. The percentage change between 1950 and 1960 is compared with the percentage change in crime rates for the periods of 1955 to 1965; 1959 to 1965; and 1960 to 1965 (1970:209). This procedure is repeated for data for certainty changes between 1950 and 1963 and between 1960 and 1963 with comparisons for subsequent changes in crime rates. This mode of analysis is repeated for the data on changes in severity and changes in crime rates. Chiricos and Waldo interpret the data as providing no support for relationships between changes in certainty and severity and subsequent changes in crime rates (1970:210).

Teevan (1972) uses data from the Canadian Crime Statistics, Statistics of Criminal and Other Offenses, and Correctional Institution Statistics, published by the Canadian Dominion Bureau of Statistics, to test for a possible relationship between the certainty and severity of legal responses and crime rates in Canada.

Teevan is one of the few researchers of deterrence to use court data. Thus, his index of certainty is the percentage of court convictions from the number of crimes known to the police (1972:159).

Teevan is also innovative in deterrence research because he uses, as a crime rate, the total number of crimes known to the police per one hundred thousand of the population aged seven or older.

Examining data for the years 1964 to 1967, Teevan concludes that there has been a general but slight decline in the certainty of convictions accompanied by a general but slight increase in the crime rate (1972:160).

Teevan then uses his index of certainty to determine the association for the specific offenses of rape, robbery, breaking and entering, and murder. The analysis utilizes the data for each offense, for each year from 1964 to 1969, inclusive. Teevan concludes that for all the offenses there is a slight decrease in certainty and a slight increase in crime rates (1972:160-161).

Teevan gauges the severity of legal response by computing the median time served by offenders released during the years under study for the offenses of robbery, rape, and breaking and entering (1972:162).

The analysis of statistics for the years 1964 to 1968 inclusive leads Teevan to infer that severity has remained constant and that crime rates have been increasing (1972:163).

Some Economic Studies

Econometricians have researched deterrence arguing that criminals can be treated as economic, rational beings, making decisions under risk. They see the choice between legal and illegal behaviour as a function of psychic and economic costs and the time invested in criminal behaviour to be partly a function of the perception of the certainty and severity of legal responses. These studies, like those in sociology, use geographic units of analysis, secondary statistics as sources of data, indexes of crime and legal responses and sophisticated statistical analysis.

Ehrlich (1972) argues that deterrence can be studied by using the assumptions that individual decisions to engage in illegal versus legal activities is a function of the gains and costs associated with these pursuits. Two of the determinants of this choice are the certainty and severity of legal responses. Increases in either of these variables, with all else being equal, are held to reduce the incentive for crime. Ehrlich also argues that certainty and severity are responded to differently by those who are risk preferers, risk neutral, and risk avoiders. Further, individual choices are subject to the opportunities to work legitimately and/or in criminal activity, the offenders capacity to bribe, employ legal counsel, the time needed to learn the required skills, and previous convictions (Ehrlich, 1972:265).

Ehrlich's approach is one which emphasizes the determinants of individual choice, but because there are no data available to measure these variables, he substitutes aggregate data for American states in order to test his theory. Using the Uniform Crime Reports and the National Prisoner Statistics as sources of data for the seven "index crimes," for the years 1940, 1950, and 1960, Ehrlich attempts to test his argument about individuals at the aggregate level (1972: 269).

Ehrlich asserts that one can separate the measurement of incapacitation effects--that is, the lowering of crime rates due to the offenders being in prison--from deterrent effects. Ehrlich states that incapacitation applies only to offenders arrested and imprisoned, while deterrence affects "all actual and potential offenders" (1972:268). He contends that incapacitation can be measured by (P) the increase in the certainty and severity of legal responses, indexed as the rates of the number of offenders arrested and punished, divided by the total number of offenders at large, or (T), the number of previous sentences served by the offender. Ehrlich refers to previously published data which indicates that for specific states these measures show "persistent and relatively steady differences." He argues further that incapacitation effects can be separated from deterrent effects by comparing the crime rates for (1) states where P and T are roughly equal in value--incapacitation--with (2) states where P and T are significantly different. He contends that when crime rates respond to differences between P and T, this indicates a deterrent effect (1972:269).

Ehrlich uses multiple regression analysis to estimate the effect of certainty and severity upon crime rates. His crime rate consists of the number of offenses known to the police per capita. (Note that Ehrlich's concept of all actual and potential offenders is inappropriately measured as offenses known to the police.) His certainty measure is computed as the ratio of commitments to federal and state prisons divided by offenses known to the police. He determines severity in terms of the average length of time served by inmates released during the year under study. In addition he points out "some of the major theoretical determinants of criminal activity" are the per cent of the non-white population, the median family income, and the percentage of families below one-half of the median family income. Further, he observes that while serious crimes are under reported and imprisonment is not the only way that offenders are treated, it is reasonable to assume that these two variables are randomly distributed in the population from time to time and place to place. His regression model for the crime rate is a function of (1) certainty of imprisonment, (2) severity of imprisonment, (3) median family income, (4) percentage of families below one-half of the median family income, (5) and an error term which is based on the assumption that the measurement error and variations among these variables are random.

Since the crime rate may be a determinant of certainty and/or severity, Ehrlich analyzes his data by excluding measures of the crime rate and determining the certainty for various determinants of criminal activity. For example, by comparing the certainty of arrest with the certainty of imprisonment separately for whites, non-whites, he believes that he has eliminated the effect of the crime rate on certainty (1972:275).

Ehrlich also argues that the effect of certainty and severity of legal responses can be due to offenders shifting from one kind of criminal activity to another. For example, he claims that because offenders arrested for robbery are often convicted of burglary, that these two offenses are complementary. On the other hand, he claims that theft is a substitute for robbery. He proposes therefore, that increases in the certainty of arrest for burglary can reduce the rates of crime for both the offenses of robbery and burglary and increase the rates of offenses for theft. Offenders can shift from offenses which are complements to those which are substitutes. To test for this possibility, Ehrlich compares the crimes against property with those against the person. (This does not provide a test of Ehrlich's plausible argument, because his complementary and his substitute offenses are within the property offense group and the crimes against the person that he studies, that is, murder, rape, and assault, are not substitutes for property offenses.)

Ehrlich interprets all of his data as supporting his contentions. He states that the magnitude and the signs of the various correlations all indicate support for the importance of certainty and severity in affecting criminal activity, and that evidence for states where P and T are about the same magnitude compared with states where P and T are very different permits the separation of incapacitation from deterrent effects (Ehrlich, 1972:275).

Sjoquist (1973) argues that the time any individual spends in criminal activities is a function of his expectations for psychic and financial gains and costs from legal versus illegal behaviour and the subjective probability of being arrested, convicted, and sentenced. However, Sjoquist points out that there are no data upon which to compute these functions for individuals. As a consequence, he replaces the hypothetical variables for individuals with aggregate data from communities.

Sjoquist selected fifty-three American communities with populations between twenty-five thousand and two-hundred thousand in 1960 (1973:441). His crime rate, based upon the Uniform Crime Reports, consists of the number of crimes known to the police for the property offenses of robbery, burglary and larceny over fifty dollars, for the year 1968.

Sjoquist uses three measures of certainty: (1) the probability of arrest as a ratio of the number of arrests to the number of offenses known to the police; (2) the probability of conviction, as a ratio of the number of convictions to the number of offenses known to the police; and (3) the probability of punishment, the ratio of the number of convictions to the number of arrests. These certainty indices are computed for the year 1968 and are based upon statistics from the Uniform Crime Reports (1973:442). As a measure of severity, Sjoquist uses the mean time served by inmates released from state and federal prisons in 1960, based upon the statistics reported in the National Prisoner Statistics.

Sjoquist points out that several demographic and economic variables affect crime rates and so he includes in his analysis the per cent of the population non-white, average education in terms of years completed, population density and the overall population (1973:443).

Sjoquist uses multiple regression analysis, with the total number of property offenses, known to the police per capita as his crime rate and dependent variable (1973:443). He uses four regression equations to estimate the crime rate. The first regression equation consists of (1) the certainty of arrest; (2) severity of sentences; (3) net gain from legal activities, as measured by either the annual labour income for manufacturing workers for 1968, *or* the rate of unemployment for 1968, *or* the percentage of families with incomes of three thousand dollars or less. The choice of which of these to use was made in terms of available data. In addition, the equation includes (4) the percentage of the population white, (5) the average level of education, (6) population density, (7) population size, and (8) a term which assumes that errors are normally distributed (Sjoquist, 1973:443).

The second and third regression equations are the same as the first except that, in the second, Sjoquist uses the certainty of convictions and, for the third, he uses the certainty of punishment as independent variables. The fourth regression equation uses both the certainty of punishment along with all the other variables (Sjoquist, 1973:443).

Sjoquist states his data indicate that increases in the probability of arrest, conviction, and punishment are associated with decreases in the amount of crime. Further, he claims that all the associations for severity and crime rate are negative. Overall, Sjoquist interprets his data as supporting an economic theory for property crimes (1973:446).

Our preamble to this chapter points out that research on deterrence leads to a variety of findings and conclusions. Further, our assertion that, to date, general deterrence has neither been confirmed nor refuted by research is illustrated by our summary of studies on deterrence. While each study has its own methodological flaws, there are major problems common to studies of deterrence. In addition, there are requirements for an adequate test of deterrence hypotheses. The remainder of our discussion attempts to explicate some of these.

FOOTNOTES CHAPTER THREE

[1] (1) Vermont maintained a rather constant rate; (2) Virginia, and North and South Carolina, experienced increases in homicide rates equal to or exceeding their high rates for the thirties; (3) by 1930, Nevada, Florida, and Michigan showed declines in homicide rates; (4) Connecticut continued to increase in homicide rates from the thirties up to 1949.

CHAPTER FOUR

HYPOTHESIS TESTING AND GENERAL DETERRENCE

Two major issues have been neglected in general deterrence studies. Researchers have not been careful about the logical and empirical requirements for formulating testable hypotheses about deterrence. In particular, the role of unobservable variables and their importance in the logic of general deterrence has been neglected. In addition, students of deterrence have not attended sufficiently to the rules for deciding whether a hypothesis is confirmed or refuted. As a consequence, while assertions about general deterrence may be psychologically comforting, in that they invoke a logic based on *prima facie* assumptions, they run the risk of circular reasoning. Thus, Sutherland and Cressey, antagonists of deterrence, claim that "when the mores are adequate, laws are unnecessary; when the mores are inadequate, laws are ineffective" (1966:11). This is tantamount to saying that if legal measures are not associated with a decrease in criminal activity, it is because they are not supported by the mores; if legal responses are associated with such a decrease, it is because they are supported by the mores and are therefore unnecessary. Such arguments are tautological, impossible to test, and add little to our knowledge of deterrence.

On the other hand, protagonists of deterrence may argue that, if crime persists despite legal responses, the responses are not certain, swift, or severe enough. Use of the term "enough" means that the assertion cannot be falsified.

When attention is directed to the empirical assessment of general deterrence, it is apparent that many assertions are not only untested, but are also untestable. In part, this is because many assertions about deterrence rest upon several kinds of unmeasurable variables.

DETERRENCE AND UNOBSERVABLE VARIABLES

"Where there is not temptation, there can be little claim to virtue" (Prescott, 1947, cited by Bartlett, 1955:484). Similarly, where there is no inclination towards crime, there can be little claim to deterrence. It seems reasonable to contend that most, if not all, beliefs about general deterrence entail the proposition that legal responses to crime will effect potential criminals in such a way that they do not become actual offenders. The testing of this proposition requires the *measurement of potential crime prevented*. More specifically, it requires evidence that persons are "*eligible*" to commit certain crimes; that some of these persons have a "*propensity*" for that crime, that is, that they are in some kind of state of "readiness;" and that this propensity is "*suppressed*." Further, of all the possible variables which might cause termination of this sequence of events, it is the *legal responses to other offenders* which, in fact, cause such a termination.

Although we can determine eligibility for crime, the measurement of propensity is problematic. (This is discussed in Chapter Five.) Further, to date, there are no criteria for measuring "suppressed" acts of criminality. Consequently, direct measurement of general deterrence is not possible.

THE LOGIC OF ARGUMENTS ABOUT GENERAL DETERRENCE

Since general deterrence cannot be measured directly, crime rates are used as an indirect and supposedly valid measure of deterrence. The argument is that if crimes are committed then deterrence has not occurred and the absence of deterrence can be inferred from increasing crime rates, and the presence of deterrence can be inferred from decreasing crime rates. This reasoning is based upon a non sequitur, since committed crimes and suppressed crimes are both empirically independent and logically asymmetrical. Whereas the former entails the latter, the reverse does not apply.[1] Thus rates of suppressed crimes cannot be read off from rates of committed crimes. Nevertheless, committed crimes are used by students of deterrence to make inferences about the presence of general deterrence, and this pattern of reasoning has been accepted and institutionalized to the point of ratification.

GENERAL DETERRENCE HYPOTHESES

An alternative approach to making inferences about suppressed crimes from committed ones may be found in the proposition that variations in legal responses to crime cause variations in crime rates. More specifically, the term general deterrence is used to refer to the proposition that changes in the certainty, swiftness, and severity of legal responses are related to the production of crime.[2]

The relationship between legal responses and crimes has been and probably will continue to be, examined by deterrence researchers in order to make inferences about general deterrence. This perpetuation involves four hypotheses.

The Actuation Hypothesis

Criminology literature suggests that legal responses may facilitate crime and that some potential offenders may actually desire and seek out legal responses. The enticement quality of legal responses has been referred to as "forbidden fruit" (Zimring, 1973:33).[3] Facilitation effects have been documented for at least one kind of crime--that of tax evasion. Schwartz and Orleans observe that, following a campaign intended to publicize the severity of penalties, the rates of income tax evasion actually increase (1967:276). The researchers attribute this phenomenon to the proliferation of knowledge about illegal tax loopholes. Some respondents reported that they started to cheat on their income tax only after they had witnessed convictions for tax evasion obtained by others in their jurisdiction. These facilitative effects may occur because of "modelling" (Nettler, 1974:4)[4] or "contagion" (Berkowitz and Macaulay, 1971).[5] For our purposes, we suggest that the claim which purports that increases in legal responses are associated with increases in crime be called an actuation hypothesis.[6]

The Attenuation Hypothesis

Another criminology argument maintains that legal responses leave the offender stigmatized and that this stigma perpetuates criminal behaviour. Labelling theory suggests such a supposition as do penologists, who maintain that some legal responses are in themselves criminogenic (Becker, 1964: 16; Brooker, 1972:484; Nettler, 1974:205). Consequently, some proponents of this approach advocate that decreases in legal responses, such as diversion and de-criminilization, are associated with decreases in criminal behaviour. This belief we will call the attenuation hypothesis.

The Abatement Hypothesis

Most of the literature on deterrence reads as if there were only one proposition involved. Yet, deterrence theory actually involves two propositions, which are probably quite different from each other. The bulk of deterrence literature argues that increases in legal responses are associated with decreases in crimes. This proposition will be referred to as the abatement hypothesis.

The Augmentation Hypothesis

Mixed with assertions about the abatement hypothesis are others which assume that decreases in legal responses are associated with increases in crime. This belief will be called the augmentation hypothesis.

Statements about general deterrence are usually based upon beliefs concerning the abatement and augmentation hypotheses. Discussions of deterrence never distinguish between these two hypotheses, and, in fact, evidence about one is often regarded as evidence about the other. The logical and empirical problems of the relationship between evidence about augmentation and evidence about abatement have not been considered by students of deterrence. In addition, there is no justification for treating actuation/attenuation hypotheses as simply converses of abatement/augmentation hypotheses. Yet it is reasonable to argue that deterrence theory is derived mostly from the belief that the former hypotheses account for less crimes than do the latter hypotheses.

It is difficult to draw conclusions about deterrence since the relevant hypotheses have not been appropriately delineated. Further, the relationships among the four possible hypotheses are obscure. The extrapolation of evidence about one hypothesis vis-a-vis the others raises issues which have not yet been confronted by students of deterrence. Finally, there has been no examination of the relationship between a hypothesis and the observations generated to test it. Decisions as to whether observations made in testing a hypothesis can be used as evidence for that hypothesis are problematic. At best, these difficulties make conclusions about the hypothesis itself tentative, and at worst they make inferences about related hypotheses erroneous.

TESTING HYPOTHESES

Any adequate test of deterrence meets with problems of the relationship between the observations and the status of the hypothesis. This difficulty leads into the area of confirmation and refutation theory. To date, however, no researcher of deterrence has adequately discussed the criteria for deciding whether his hypothesis is "true or false."

It is generally held that two approaches to research permeate the social sciences; one approach seeks the confirmation of a hypothesis, and the other is designed to refute a hypothesis (Wallace, 1971:77-85; Bunge, 1973:27-31). These two approaches, which differ in procedure, are, however, identical in two ways. Both use testability as the essential criterion, and both examine the criteria which determine whether the test results confirm or refute the hypothesis.

Confirmation Theory

Confirmation is a term by which social scientists refer to the process of deducing hypotheses from theories and then subjecting them to "test." Testing involves assertions about what should be observed if the hypotheses were "true" and the subsequent generation of observations. If the expected observations occur, then the hypotheses are believed to be confirmed (Zetterberg, 1965; Wallace, 1971:63-85; and Salmon, 1973). This approach to hypothesis testing in social science in general, and deterrence research in particular, confronts two major issues: (1) the relationship between the observations and the confirmation of hypotheses, and (2) the implications of the property of transitivity for the deductive syllogism.

Do Consequences Confirm?

Amongst the numerous traps associated with testing a hypothesis is the confusion of logical validity and empirical observation. This confusion concerns the belief that deducing valid conclusions from premises is the same as predicting consequences from hypotheses. It is the belief that, if expected observations made in testing hypotheses occur, then the hypotheses are true (Salmon, 1973:77). This problem of confirmation is outlined by Blalock as follows:

> Evidence for the original theory is indirect. The theory A implies certain consequences B, or written symbolically, A ⟶ B. It should be emphasized that purely logical or deductive reasoning rather than empirical evidence is used in going from A to B. Therefore, if A is true, B must also be true, provided our reasoning in deducing B from A is valid.
>
> We then look to see whether or not B has occurred; if B has not occurred (B false), then we know that theory A must also be false.
>
> But what if B turns out to be true? Can we conclude that A must be true? We cannot. If we do, we shall be committing the "fallacy of affirming the consequent" as logicians refer to it. If B is true, we can say that A may be true, but there could be any number of alternative theories which also predict B. (1960:8)

Blalock shows that observation, or finding a "positive instance," in no way leads automatically to the verification of a hypothesis. Salmon also shows how expected observations can lead to absurd conclusions (1973:77).[7] In short, confirmation involves more than the discovery of consequences.

The Transitivity of Logical Entailment

Transitivity is another basic problem of confirmation. Salmon argues that the transitivity requirement of deductive syllogisms seriously attenuates the possibility of confirmation.

He asserts that

> . . .logical entailment has the obvious and important property of transitivity such that if A entails B, and B entails C, then A entails C. If deductability of true consequences were the whole story regarding confirmation, then it too would be a transitive relation. If C were to confirm B, because it follows from B, and if B were to confirm A because it follows from A, then C would have to confirm A, since C would follow from A by transitivity of deductive entailment. (1973:78)

Salmon (1973:78) illustrates, however, that transitivity may not only fail to confirm a hypothesis, but may lead to conclusions which are incompatible.[8]

Refutation Theory

The refutation approach to hypothesis testing is explained and defended by Popper (1961). He argues that hypotheses should be deduced from theory and then subjected to tests designed to show that they are false. Research should attempt to overthrow a theory by refuting a hypothesis which is crucial to its logical structure (Popper, 1961:87). This approach uses criticism and involves pragmatic, rather than logical, criteria; knowledge of how to test a hypothesis and gather evidence; and "ingenious and honest" efforts at falsification (Giedymin, 1964:55). Refutability for Popper is the essential criterion of science (Bunge, 1973:27).[9]

Rules for Confirming and Refuting Hypotheses

Neither the confirmation nor the refutation approach to hypotheses testing is free from problems. For example, hypotheses testing often rests upon the acceptance of auxiliary hypotheses--the acceptance of implicit hypotheses or the assumption that certain initial conditions are satisfied. The confirmation or refutation of a hypothesis may therefore be due to the "truth" or "falsity" of these auxiliary hypotheses (Salmon, 1973:81).[10]

Hypotheses testing requires decisions about whether the testing procedures worked or failed, or whether the results confirm or refute the hypothesis. Unfortunately, criteria for making such decisions are not clear (Salmon, 1973:83; Bunge, 1973:28). Thus, Salmon suggests that alternatives to assuming that confirmation is guaranteed by predicting consequences and the problem of logical transitivity are found in probability theory and the idea of incremental confirmation (1973:83).[11]

Probability theory can aid in decisions about hypotheses testing by determining the "probability that certain evidence would obtain if a particular hypothesis is true..." and then "...computing the probability that the hypothesis is true given that the aforementioned evidence is found" (Salmon, 1973:83).[12]

Incremental confirmation consists of reasoning that, (a) if a test of a hypothesis results in evidence for the hypothesis, then we can say that this evidence raises the probability of accepting the hypothesis, and that, (b) the hypothesis is "confirmed" if a large number of tests all result in evidence for the hypothesis (Salmon, 1973:76). Increases in the probabilities allow for increases in the credibility of the hypothesis.

Platt argues that the most effective way of testing hypotheses and deciding about their proof is by using the method of analytic inductive inference which he calls strong inference (1964:347-353). This method consists of applying the following steps:

> 1)Devising alternative hypotheses:
> 2)Devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses:
> 3)Carrying out the experiment so as to get a clean result:
> 1')Recycling the procedure, making subhypotheses or sequential hypotheses to refine the possibilities that remain: and so on. (1964:374)

While the adequate testing of general deterrence probably requires the confirmation and/or refutation of at least two hypotheses--abatement and augmentation propositions--and possibly four hypotheses--abatement, augmentation, actuation, and attenuation propositions--it is clear that researchers on deterrence, (a) have not delineated all the relevant hypotheses, nor (b) have they specified the logical and empirical relationships among them, and (c) they have not attended to the initial step of considering the rules for deciding that a hypothesis is confirmed or refuted by the evidence generated to test it. Finally, the possibility of devising "crucial experiments," in order to exclude any of the hypotheses, seems remote. Research designs used to test general deterrence hypotheses do not appear to be adequate for refuting and therefore, excluding hypotheses. However, before the research designs are evaluated, we turn to the problem of the measurement of crime for the study of general deterrence.

FOOTNOTES CHAPTER FOUR

[1] Ball notes that:

> . . .it would be inaccurate to consider a legal enactment. . .as an effective deterrence solely because the law was rarely violated. Such a situation might merely indicate that the enactment was unnecessary or that the number of potential violations was negligible. Conversely, the fact that a considerable number of persons violate a general preventive measure is not in itself sufficient evidence to demonstrate its failure as a deterrence. (1955:350)

Van den Haag points to the problem of interpreting crime rate data about deterrence. He states:

> Sellin seems to think that this lack of evidence for deterrence is evidence for the lack of deter[illegible]e. It is not. It means that deterrence has [illegible]een demonstrated statistically--not that non-deterrence has been. (1967:285)

Bedau concludes that:

> . . ..the positive instance of successful deterrence by any penalty cannot be read off directly from any data available. All evidence for general, special, and marginal deterrence must be indirect. (1973a:16)

[2] Osborne states that deterrence "is a consequence more specifically of the relationship between crime and punitive reaction" (1968:17). Teevan notes that researchers

> . . .usually attempt to test the long-standing hypothesis of Beccaria (1764) and Bentham (1843) that celerity, severity, and certainty of punishment are inversely related to the incidence of crime. (1972:155)

[3] Although Zimring points out that such consequences are not inevitable, he also notes:

> Although to some observers, existence of the threat as a barrier to committing a particular act causes members of the threatened audience to revise attitudes towards the desirability of the threatened

> behaviour. People who felt that the threatened behaviour was desirable in the absence of the threat will consider the behaviour more desirable, even if they are persuaded by the prospect of consequences to heed the threat. To those who originally were neutral about the desirability of a threatened behaviour, it will acquire some positive value, apart from the unpleasantness of threatened consequences (emphasis mine). (1971: 33)

Bedau suggests that "executions, far from operating solely as a deterrent, actually incite some persons to criminal violence" (1967:264). The evidence offered for this contention is, for the most part, limited to psychiatric anecdotes. Ziferstein, a psychiatrist, is quoted by Bedau as follows:

> A couple of. . .recent cases come to mind. One the case of a disc jockey. . .who shot a complete stranger on the streets of Los Angeles in broad daylight. On being arrested, he stated that he had been despondent for some time but did not have the courage to commit suicide. He therefore chose murder as an indirect route to his own destruction. A similar case occurred. . .where a man, after several unsuccessful attempts at suicide, killed his landlady and turned himself in. . . The State eventually obliged him by executing him. For every case where this mechanism of suicide-by-murder is conscious and is verbalized, there are probably several where the same mechanism is unconscious and not manifestly verbalized, although it can be deduced from a careful study and interpretation of the material. In these cases, obviously, the death penalty is not a deterrent, but has quite the opposite effect of motivating the sick person to commit murder. (1967:264)

Schwartz and Orleans report some historical data of an extreme example:

> Orsted describes a period in eighteenth-century Norway when homicide rates increased because of the belief of suicidal persons that execution for murder was the only technique of suicide which might excuse them from eternal damnation. This led to the abolition of the death penalty in such cases on the ground that it acted as an incentive to, rather than a deterrent from, murder. (1967: 278)

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[4] Knowledge of the criminal life style as a alternative

may tend to actuate crime rather than deter it:

> Thus, the first American skyjacker to parachute with an extorted fortune immediately became a type of folk hero, his name emblazoned on T-shirts, and his acts imitated by others. (Nettler, 1974:4)

[5] Some work has been done on this facet of the actuation hypothesis. Berkowitz and McCaulay (1971) suggest that when the administration of severe sanctions is accomplished by considerable publicity, the incidence of the act may increase due to suggestion and imitation. They attempt to document the contagious effect following the assassination of Kennedy in 1963 and the mass murders by Speck and by Whitman in 1966.

[6] My usage of the word "actuation" is suggested by the dictionary definition of the verb "to actuate" which means "to incite or to put into action" (Barnhart and Stein, 1964:13).

[7] Salmon uses the following syllogism to illustrate how inappropriate combinations of logic and observation can lead to absurd conclusions:

> Consider the hypothesis "pigs have wings." In conjunction with the observed fact. . .that pork is good to eat, we deduce the consequence--we predict--that some winged creatures are good to eat. When we see that people enjoy eating ducks and turkeys, we observe that the consequence--or prediction--is true; we appear to have a confirmation of the original hypothesis. (1973:77)

[8] Salmon uses the following syllogism to illustrate how the property of transitivity can lead to conclusions which are incompatible with the hypothesis:

> Since it is very probable that any scientist that ever lived is alive today (it has been estimated that 90 per cent of all scientists are still alive), and since it is very probable that any organism alive today is a micro-organism, then, given that Smith is a scientist, it is likely that he is a micro-organism. This example shows unmistakably that A (being a scientist) can lend confirmation to B (being alive at present), and B can in turn lend weight to C (being a micro-organism), where A not only fails miserably to

> confirm C, but also is actually incompatible with it. (1973:78)

[9] The refutation approach to hypothesis testing and deterrence is exemplified by the quasi-experimental approach to research. Campbell and Ross (1963:205) see the role of research in terms of refuting possible rival hypotheses. The task of theory testing and data gathering consists of rejecting hypotheses which are inadequate. Research, for these authors, "probes" but does not "prove" theory. However, the greater the number of plausible rival hypotheses that are refuted, the greater the degree of confirmation for the hypothesis under test:

> . . .varying degrees of 'confirmation' are conferred upon a theory through the number of plausible rival hypotheses available to account for the data. The fewer such plausible rival hypotheses remaining, the greater the degree of confirmation (Campbell and Ross, 1963:206).

[10] For example, Salmon (1973:81) states that testing astronomical hypotheses by using telescopes and cameras rests upon acceptance of the laws of optics.

[11] Salmon suggests that Bayes' theorem is a possible way of computing the probability that a hypothesis is true. He states:

> Given the probability that certain evidence could obtain if a particular hypothesis is true (and given some other probabilities as well), Bayes' theorem enables one to compute the probability that the hypothesis is true, given that the aforementioned evidence is found. In at least certain cases, it can be used to ascertain that some particular cause was operative, given that a certain effect has occurred. . . Bayes' theorem contributed to confirmation theory a scheme that seems far more adequate to inference in science than the fallacy of affirming the consequence can ever hope to be. (1973:83)

Bayes' theorem (Salmon, 1973:79):

$$\text{Probability (A, given B)} = \frac{\text{Probability (A) X Probability (B, given A)}}{\text{Probability (B)}}$$

[12] Salmon proposes a partial solution to the problem of logical entailment in terms of the probability analogy:

> "All" and "Almost All" can be very different. Given that all A are B, and all B are C, it must be true that all A are C. . . However, given that almost all A are B, and almost all B are C, it may happen that no A are C. . . Although most scientists (A) are living things (B), and most living things (B) are micro-organisms (C), no scientists at all are micro-organisms. (1973:79)

CHAPTER FIVE

CRIME RATES FOR GENERAL DETERRENCE HYPOTHESES

Difficulties in inferring conclusions from data are only part of the trouble in thinking about deterrence. There remain questions about the quality of the data themselves. For the formulation of general deterrence hypotheses we need the reliable and valid measurement of crime over time. A crime rate measurement over time is required which consists of a numerator, representing units of crime, divided by a denominator, representing the population-at-risk, within a given time period.[1] Since deterrence literature contains varying applications of crime rates, as well as varying formulations of the numerator and denominator, it is important to analyze some characteristics of these elements in order to avoid ambiguity.

THE NUMERATOR

Crime rates have been measured from different vantage points for different purposes and it is difficult therefore to define their common basis.[2] It is not the "correctness" of alternative crime rates, but their applicability to specific problems, which requires definition. In order to establish an accurate numerator for the determination of a realistic crime rate, we must first attempt to identify three elements: (1) Should the numerator express the number of offenses or the number of offenders? (2) If we employ offenders, should the numerator express offenders in toto or specific kinds of offenders? (3) Should the numerator express the frequency or the gravity of crime, or both?

Offenses Versus Offenders

Crime rates may be expressed either by the number of offenses committed or by the number of offenders. The latter method appears to be more accurate for use in general deterrence studies. Preference for the use of the "offender rate" is based on such factors as: (1) general deterrence concerns the effects of legal responses on persons, and (2) information from the offender is often required before the offense itself can be established (for example, to distinguish murder from manslaughter).[3] Furthermore, by employing an "offender rate," we can exclude juveniles and the insane,[4] since the philosophy of legal responses for these persons is not based on deterrence. For example, juveniles cannot be expected to be affected by the usual legal responses to crime, because the separation of the juvenile court from the adult court guarantees that, in almost all cases, the consequences of juvenile "crime" will be different from the consequences of adult crime. This basic differentiation is reflected in the common language usage, in which illegal behaviours committed by juveniles are considered to be delinquencies and qualitatively different from criminality proper. The statistics available on criminal offenses, basically the standard reporting of crimes known to the police, obviously do not provide any way to differentiate juvenile crime from adult crime.

This would present only a minor problem if it could be assumed that children commit very few crimes, and so contribute only an insignificant amount to the total known crime in our society. Unfortunately, such is not the case. Christensen estimates that almost a quarter of all arrests are for persons under 18 years of age (1967:218). Biderman notes that 20 per cent of all larcenies involve bicycle theft (1966:119). A very large proportion of thefts over $50.00 are auto thefts. Biderman notes further that of the few offenders who are charged with auto theft, more than half (58 per cent) are juvenile joy-riders (1966:121). However, a simple examination of the total number of cars reported stolen provides no information on the proportion stolen by amateur adolescent joy-riders and that stolen by a professional adult car thief.

Total Offenders Versus Offense-Specific Offender Rates

Beccaria has argued that legal responses for minor crimes will produce a "generalizing effect" so that serious crimes will also be deterred (Manzoni, 1964:57). Contemporary students of deterrence, however, have rarely proposed a crime rate based upon total offenses or offenders. Most studies use "offense-specific" crime rates and, to date, no one uses "offender-specific" rates. While "total-offender" rates would be in keeping with Beccaria's idea of a generalizing effect, "offender-specific" crime rates also provide a reasonable measure for testing deterrence hypotheses.

Initial Offenders

If we assume general deterrence to be "offense-specific," we find a revealing indicator of its applicability in the rate of initial offenders. This group includes both first offenders, that is, those who had previously committed neither the offense in question nor any other offense, and new offenders, that is, those who had not previously committed the offense in question but had committed other offenses.[5] A basic concept in law expresses the view that recidivist offenders differ from initial offenders in that they are subject to specific deterrence and, therefore, require a different response.

The number of persons who could be excluded under this criterion is difficult to estimate, given the paucity of data. Christensen is one of the few criminologists to attempt to measure crime in terms of the percentage of population which has a record of arrest for a non-traffic offense, and the percentage of the population convicted of such an offense (1967:216). Christensen's use of "virgin arrest ratio" and "virgin conviction ratio" could be applied to the study of initial offenders (1967:216). He suggests that almost all estimates of the proportion of offenders who are "virgin offenders" are incomplete and probably high. Problems involve decisions on whether juvenile arrests count as prior records. He concludes that the best estimate of all adults arrested in 1965 without a prior arrest is 13 per cent, and that the probability of a lifetime arrest for males is about 40 per cent and 10 per cent for females (Christensen, 1967:221). On the basis of such estimates, it seems possible that the exclusion of initial offenders from the population-at-risk may make a significant difference in the computation of crime rates. However, researchers to date appear not to have distinguished between initial and recidivist offenders in measuring crime rates for the analysis of general deterrence (Cousineau, 1973:153).

Serious Initial Offenders

The evaluation of general deterrence can be limited to the impact that legal responses have on serious crimes. Coincident with public interest, these crimes have been the traditional concern of deterrence studies. Various researchers have attempted to determine which crimes are believed to be serious through assessing the opinions of the general public,[6] persons involved in the administration of justice,[7] and university students.[8] Legal definitions and descriptions of criminal events have also been examined.[9]

The Uniform Crime Reports, published by the F.B.I., are based on a Standard Classification of Offenses, which consists of a list of 26 offenses rank-ordered in terms of seriousness from criminal homicide to vagrancy. However, to take into account the problems of differential reporting, the Uniform Crime Reports base their calculations of crime rates on what they call "index crimes," which are those crimes believed to be well known to the police and of a relatively serious nature. "Index crimes" include criminal homicide (except negligent manslaughter), forcible rape, robbery, aggravated assault, burglary, larceny of property over $50.00, and auto theft (Reckless, 1973:63-73).

It has been suggested that the seriousness of crime is a rather consistent and stable concept in society (Rose and Pell, 1955:259) and that the public considers serious crimes to be "collective and individual violence, robbery, rape, arson, burglary, high-jacking, drug addiction, and moral decay" (Nettler, 1974:36). It appears reasonable, then, to evaluate general deterrence in terms of the rates of initial offenders who commit serious crimes.

Frequency Versus Gravity of Crime

A reliable measurement of crime for the purpose of evaluating deterrence should distinguish between the number of offenses and offenders and the gravity of offenses. It is possible that legal responses which affect frequency of crime do not correspondingly affect the gravity of crime.

Gravity refers to the relative harmfulness within a given offense category;[10] its measurement has received little attention in criminology, except perhaps in the work of Sellin and Wolfgang (1964). There appears to be only one deterrence study to date which has considered the gravity as well as the frequency of offenses. Schwartz (1968:513) suggests that legal responses not only affect the number but also the gravity of offenses. To test his hypothesis, Schwartz examines trends in forcible rape for several months before and after increases in the legislated penalties for this offense. Using the Sellin-Wolfgang scale, Schwartz categorizes the gravity of cases on the basis of two criteria: (1) whether the rape resulted in bodily harm and (2) the extent of bodily harm as measured by hospitalization (Schwartz, 1968).

A second research project, although not concerned with deterrence, considers the gravity and frequency of crime (Normandeau, 1969). Normandeau examined a random sample of robberies, using police data, and gives each criminal incident a rating on the Sellin-Wolfgang scale. He is, therefore, able to make comparisons between trends in the frequency of robberies as measured by data from the Uniform Crime Reports, and trends in the gravity of robberies as measured by the Sellin-Wolfgang scale. The study indicates that changes in frequency are not necessarily related to shifts in gravity.

Frequency of crime can be measured by (a) the number of convictions or (b) the number of arrests, or (c) the "crimes known to the police." Some students of deterrence consider only number of convictions as measures of crime (Tappan, 1969; Giffen, 1965). While this approach is legally proper, it is too restrictive to be used for the study of general deterrence. Sellin (1931:341), noting that conviction rates represent the end of a series of steps influenced by a number of nonlegal factors, argues that the "best" crime rate is that procedurally closest to the phenomenon of interest. That is, if one is interested in investigating rates of offenses, then crimes known to the police constitute the best index; if one is interested in determining the rates of offenders, then arrests form the best index (Sellin and Wolfgang, 1964:59).[11]

For the evaluation of general deterrence it seems appropriate to use the arrest rate for serious initial offenders as an index for frequency of crime. In addition, arrest data are believed to be more reliable than court data, especially when serious crimes are considered (Wolfgang, 1958:173; Turk, 1969; Ferdinand, 1970). To date, however, no studies of deterrence have employed this rate.

It is known that the amount of actual crime exceeds that recorded as offenses known to the police. There is considerable agreement that inhibiting factors, such as knowing the offender, are responsible for the discrepancy between actual crime and its recording, but we do not know if these factors operate consistently, either in degree or direction. This lack of information may not interfere significantly with crime counts in general, yet it presents a serious problem in the evaluation of deterrence.

Some studies suggest that the amount of actual crime may be twice the amount reported and victimization studies suggest that the incidence of crime may be from three to ten times the amount recorded by police (Biderman, 1967:17; Walker, 1971: 15-28). In a large number of cases, the police have no knowledge of crimes because the victims do not report them. Recent studies in victimology suggest a number of reasons for this (Biderman, 1967; Biderman and Reiss, 1967; McIntyre, 1967; Hawkins, 1973). Biderman (1967:21-28), for example, lists among the variables which tend to reduce the probability of an offense being reported, simple negligence; the victim feeling that he may have contributed, at least in part, to his own demise; embarrassment; knowing the offender; fear of reprisals; fear of becoming a suspect; fear of the cancellation of insurance; the thought that reporting the offenses would be a waste of time; the notion that the victim did not have sufficient evidence; and finally, uncertainty as to the correct course of action. Although it seems reasonable to assume that serious offenses will be reported frequently, studies in victimology reveal that some persons fail to report such serious crimes as forcible rape and murder (Biderman, 1967).

Even if complaints are reported to the police, they are not always recorded by police. Black finds that two-thirds of encounters between police and complaints results in official field reports (1970:735). Police discretion in this matter seems to rely, in part, on the nature of the face-to-face encounter and on the social characteristics of the complaint. Black suggests that the detection of the offender may often depend less on his crime than on the identity of his victim (1970:747).

Furthermore, not all official field reports become "crimes known to the police" because of such factors as the "unfounding" process. Unfounding occurs when the police decide that a reported offense did not actually take place or that there is not enough evidence of an offense for them to proceed. For example, a discussion in the University of Pennsylvania Law

Review (1968) suggests that, compared with other serious violent crimes, forcible rape and attempted forcible rape are more likely to be unfounded by the police. The Uniform Crime Reports suggest that the national average of unfounding complaints of rape is about 20 per cent. However, the Review reports that from 75 to 90 per cent of cases are unfounded. Variables which are associated with the tendency of the police to discount claims of rape include such factors as: time interval from the supposed offense to the reporting of it; behaviour of the complaint before the supposed offense, including her social relationship with the accused rapist and her previous "chaste" or "unchaste" character; the physical condition of the complainant, including especially obvious physical harm resulting from overt resistence; the possession and/or use of a weapon by the accused; and the race of the participants, with the probability of unfounding being highest if both participants are black.

Another possible factor preventing complaints being recorded as "crimes known to the police" is fraud and deception in police records (Bell, 1960:153; Savitz, 1967:72). For example, Skolnick discloses in a study that the police manipulated the number of known burglaries (1967:173). By omitting approximately 20 per cent of field reports from their statistics, police officers obtained a lower ratio of "crimes known" to "crimes cleared by arrest" and thus raised their own apparent rate of efficiency.

Field reports which become "offenses known to the police" may not result in police action because of the size of the police force, limited resources, budgetary restrictions, and varying policies on the use of arrest (McDonald, 1969:234; Zimring and Hawkins, 1973:331; Reckless, 1973:20).

Since it is impossible to know the extent of criminal behaviour, deterrence hypotheses are not tested by assuming that a constant proportion of all crimes are unreported and unrecorded. If one accepts Walker's estimate that half the number of crimes remain unreported (1971:15-28), and if that proportion can be assumed to be constant over time and location, then assertions about increases or decreases in crime would be plausible. As Quetelet observes, statistics of crimes "would be of no utility if we did not tacitly assume that there exists a nearly invariable relationship between offenses known and adjudicated and the total unknown sum of offenses committed" (Wolfgang, 1963:713).

Unfortunately, a more plausible contention is that such an assumption is debatable. While it may be possible to identify the factors affecting the measurement of initial offenders, there is no reason to assume that they remain constant or continue to operate in the same direction.[12] In summary, as long as the actual amount of crime remains unknown, and as long as the relation between tallies of crime and the real amount of crime cannot be specified, it is impossible to evaluate the deterrent effect of an increased certainty of detection.

Victimization Studies

It has been suggested that victimization studies could improve our measure of crime for the study of deterrence (Zimring and Hawkins, 1973:332). Unfortunately, such information would be of little value for the study of general deterrence. At best, such studies might tell us how many people think they have been victimized. However, there is a distinction between estimates of offenses and offenders. For the study of general deterrence, the relevant data are the number of persons who become initial offenders for a specific crime. Only by examining offenders is it possible to differentiate between crimes committed by initial offenders and those committed by recidivists, whose criminality is a measure of specific deterrence but not of general deterrence. Victimization studies provide neither such data nor the basis for computing a crime rate over time.

THE DENOMINATOR

The second major variable in the establishment of a crime rate appropriate to the evaluation of general deterrence is the denominator.[13] If the assumption is made that general deterrence affects everyone, then the population-at-risk would be the entire population, but if only segments of the population are eligible to commit serious crimes, then it is possible the denominator can be refined. Further, if it is assumed that only certain segments of the population have a proclivity for serious crimes, then the denominator can also be modified accordingly.

Eligibility

One obvious criterion of eligibility is place of residence.[14] To be deterred, one must live within an area subject to the legal responses. Depending on the offense in question, this area may be a nation,[15] a province,[16] a municipality,[17] a city,[18] a judicial district, or a police precinct (McClintock and Avison, 1968). A second criterion of eligibility is adult status. To define the basic population-at-risk, one would exclude children;[19] generally, the legal age of responsibility is the cut-off point.[20] A third criterion requires the adult to be mentally competent and, therefore, excludes at least those institutionalized for mental illness (Turk, 1969:19). Since deterrence is offense-specific, a fourth criterion requires the exclusion of those whose individual characteristics make them ineligible to commit a specific kind of crime; for example, females cannot commit rape, and males cannot have abortions.[21] Finally, since general deterrence concerns initial offenders, the denominator must exclude all who have a previous record for the offense in question.

Proclivity

Tolstoy claims that the "seeds of crime" are in all of us (Nettler, 1974a:vii). If this is true, then the whole of society is capable of criminal involvement in all forms of crime, and the possibility of crime can be assumed without measurement. However, a more realistic contention is that only a proportion of the population has a "proclivity," "inclination," "propensity," "predisposition," or "predilection" for criminal acts.[22]

Determining Proclivity

Banfield (1968) suggests that an individual's "proneness" to crime depends upon a combination of two variables: his "propensity" to crime, which is a function of preconventional morality, a short time horizon, and low ego strength; and his "incentive" to crime, which is determined by situational factors. Zimring and Hawkins present a typology which is a partial solution to the determination of proclivity (1968: 104). The criminal category consists of persons who participate in a given form of criminal behaviour; the criminal group comprises those persons within the criminal category who are known and on whom we have information concerning their social and psychological attributes; the marginal category consists of those who are the next most likely to commit the crimes in question; and the marginal group is the identifiable portion of the marginal category, similar to the criminal group in psychological and social attributes.

The Criminal Category and Group

It seems reasonable to assert that our knowledge of the criminal category is scant. Studies of hidden criminality tend to focus on delinquents with little information about undetected adult criminals (Nettler, 1974a:62-92; Mack, 1972:44-54). Our knowledge of the criminal group is somewhat more complete. Consequently, with some variation from one type of crime to another, there is evidence to suggest that, for serious crimes, the probability of becoming an offender known to the police is related to sex,[23] age,[24] race,[25] rural/urban residence,[26] and region.[27] For any specific crime, a combination of these factors can result in large differences in the crime rate.[28]

The Marginal Category and Group

There have been efforts to measure the marginal group, that is, to locate persons who are similar to known offenders. This is the tradition of locating natural crime areas and has been discussed by Cousineau (1973:155). More problematic is the identification of those who are the next most likely to commit acts of criminality. Marginal category analysis is based upon predicting from information about the criminal group. For example, Monahan (1960) and Wilkins (1960) use age to predict the proportion of children who become juvenile delinquents. Taking into account variations by cohort size, on the basis of data from 1949 to 1954, Monahan estimates that nearly 15 per cent of all children (22 per cent of the boys and 6 per cent of the girls) will come to court on delinquency charges at least once before reaching the age of eighteen. Wilkins (1960) approaches the same kind of problem in his study of delinquency among 12 age cohorts of children born in England, Scotland, and Wales from 1938 to 1949.

In addition, the use of ecological variables for the prediction of criminal behaviours has been used extensively by researchers concerned with juvenile delinquency. Cartwright and Howard provide an overview of the tradition of delinquency-area research beginning with Shaw's work over 40 years ago (1966:321-331). Having provided this background, they then use the Borgatta-Haddan typology of census tracts in Chicago in 1960 to predict both the occurrence and kind of delinquency. Turner (1969) considers the natural crime areas associated with juvenile delinquency. Using the technique of predictive-attribute analysis, he researches census tracts in terms of a number of population variables, and is able to predict tracts that are high in delinquency rates.

While these techniques do improve our abilities to predict who will become a known offender, they do not yet disclose members of the marginal category who remain anonymous.

SUMMARY

If we assume that the determination of a realistic crime rate requires all the criteria outlined above, then no study of general deterrence has yet employed an appropriate crime rate. Most studies use the number of crimes "known to the police" as numerator, but this number does not distinguish between initial and recidivist offenders, nor does it allow for the exclusion of juveniles or the insane. Although arrest rates allow the formulation of offense-specific crime rates, and although most studies have been concerned with serious offenses, offender-specific arrest rates have never been produced. Similarly, the majority of deterrence studies uses the entire population as the denominator in determining the crime rate. As a result, many rates are given in terms of the number of crimes per hundred thousand of the population, without excluding children, the institutionalized, recidivists, or sexes when the crime in question is sexually-specific (Cousineau, 1973:154). Despite the contributions of Banfield (1968) and Zimring and Hawkins (1968), students of deterrence have not attempted to compute a crime rate with a denominator consisting of those "most likely" to commit the offense (Cousineau, 1973:155). No study appears to have used a denominator focusing on the population-at-risk and taking into consideration factors of eligibility and/or proclivity. Thus, so far neither a numerator nor a denominator has been established which might produce a realistic crime rate to be used in testing the hypotheses of general deterrence.

While the study of general deterrence is impaired by the lack of adequate measures of crime, students of deterrence have attended even less to the requirements for measuring appropriate legal responses. It is to this problem that we now turn.

FOOTNOTES CHAPTER FIVE

[1] The concept of rate is ubiquitous in social science, and is simplistic in terms of its components (Blalock, 1960:30). Basically, a rate consists of a numerator of units of behaviour, divided by a denominator of a population-at-risk to commit those behaviours, considered over a fixed period of time (Nettler, 1974a:58).

$$\text{RATE} = \frac{\text{number of observed units of behaviour}}{\text{number of persons at risk to commit such behaviours}} \times \text{Time}$$

Conventionally, as an addition to the comparability of different rates, the number of units of behaviour in the numerator is calculated per 100,000 of the base population in the denominator, and the time period is taken as one calendar year.

[2] Crime rates are defined and used in a multiplicity of ways, with resulting confusion, inconsistency, and lack of comparability. As McDonald notes:

> As there is no generally accepted crime rate, it is necessary to use a range of commonly used indicators... Going from the most serious to the least, these are: convictions for indictable offenses; charges for indictable offenses; convictions for Criminal Code summary conviction offenses; children adjudged delinquent; all summary conviction convictions; traffic convictions; parking offenses known to the police. (1969:6)

None of these measurement strategies is intrinsically more "correct" than the others, but all may more-or-less be appropriate in considering diverse problems.

[3] For almost all crimes, the legal definition of an offense depends, in part, on the intention of the offender to break the law. Unfortunately, if one examines only offenses, it is impossible to determine whether or not the offender committed them deliberately, with mens rea, or whether there were extenuating circumstances involved. Most relevant for the study of general deterrence is the confusion over the various kinds of homicides. On the basis of evidence from a corpse, a coroner can usually determine whether the person met his end by natural means or by foul play. However, having decreed that a particular

death is a homicide, he has no way of knowing what kind of crime (that is, murder or manslaughter) was committed.

[4] Legally, in order to be eligible to commit a crime, one must be mentally competent. Acts committed by persons who are judged mentally retarded or insane cannot be counted as crimes, in that such persons cannot be held responsible for their behaviours.

[5] For example, Devlin in considering the importance of an offender's record in determining the sentence he is likely to receive, notes that

> . . .a person convicted of shopbreaking who has a history of "breaking" offenses is almost certain to receive a more severe sentence than a man convicted of a similar offense in the same court who has one previous conviction for dangerous driving, not because a conviction for dangerous driving is properly considered as less serious, but because it is altogether different. . . . The difficulties occur. . .in intermediate stages where, for example, the man with a "mixed bag" of convictions is convicted of storebreaking for the first time. . . . It would seem appropriate to group together offenses which could roughly be described as involving dishonesty or violence, and perhaps revenue offenses. Because of their very nature sexual offenses present greater difficulty: is unlawful sexual intercourse "similar" to buggery or not? (1970: 47-49)

[6] Parrett (1939) has 100 citizens of New York State use the Thurstone method to rank-order the seriousness of 110 offenses. Rural and urban citizens generally show consistency in their ratings, and are able to scale the 110 offenses on an eleven-point scale. Parrett introduces an "index of ambiguity," which reflects the amount of agreement concerning the appropriate seriousness score. It is note-worthy that, the more serious an offense is considered to be, the less ambiguity there is concerning it. More recently, in 1960, the BBC asked members of the general public to select the "worst crimes" from 15 offenses, and report a rank-ordering from indecent assault (selected by 25 per cent of the sample) to assault causing grievous bodily harm (selected by 3 per cent of the sample) (cited by Rose, 1966:415). Gibbons (1969), using a non-random sample of 320 citizens in the San Francisco area, asks them regarding the sanctions suitable for offenders involved in 20 different kinds of criminality, from murder to homosexuality. The relative seriousness of the offenses can

be inferred from the proportion of the sample preferring sanctions from execution to no penalty at all.

[7] For example, Rose considers 13 crimes, from fatal stabbing to pickpocketing, and compares the seriousness ratings of students, police officers, and juvenile court judges (1966). Hartley, Rosenbaum, and Snadowsky (1967) have psychotherapists rank-order crimes, and finds that the most serious are those considered to have irreversible effects on others, such as homicide, rape, violation of civil rights, and driving while intoxicated. The least serious crimes involved minor laws, such as person's property, minor violations of individual rights, and taking advantage of others. Therapists see some behaviours commonly considered as crimes, such as homosexuality, gambling, prostitution, and disorderly conduct, primarily as the result of personality disorders.

[8] Thurstone (1927) uses students and the method of paired comparisons to rank-order the relative seriousness of 19 criminal offenses. Coombs (1967) replicates Thurstone's study and finds the rank-ordering of seriousness remains essentially unchanged, with the exception of sex offenses, which are judged to be somewhat less serious, and offenses against the person, which were judged somewhat more serious. Rose and Prell (1955) use the same procedure to have students rank order 13 felonies and report a "remarkable consistency" in the order indicated by different groups of students, and by the same students at different points in time.

[9] For example, Green considers the rank-ordering of seriousness of crimes in terms of the maximum sentence allowed by statute, and reports a high degree of correspondence between these rank-orderings and the order based on the severity of the sentences actually imposed (1961:32).

[10] The problem of assessing the gravity of crimes is complicated when attempts use legalistic definitions of crime, rather than behavioural kinds of crime. Rank-ordering becomes very difficult, and the distinctions among degrees of seriousness of similar crimes are often subtle. The method used by Chief Justice Rugg for differentiating among recklessness, negligence, and criminal negligence is instructive: it is simply the difference "among a fool, a damned fool, and a god-damned fool!" (Rugg cited by Christie, 1964:899)

[11] For example, Sellin and Wolfgang conclude:

> It is now a well-accepted doctrine that only certain kinds of offenses can be assumed to come to the knowledge of police agencies with sufficient regularity, so that changes in their number, when reduced to rates, would mirror changes in the total and partly unrecorded criminality involved. (1969:2)

[12] Many more thefts occur than are actually reported, but there are reported thefts which did not "occur," because they were made to collect insurance money. More rapes may occur than are actually reported, but some women cry rape and mean "consent followed by regret." Until the circumstances affecting the probability of arrest of an initial offender are clear, and until both the extent and the direction of their influence for a specific crime are known, it is impossible to ascertain whether variations in arrest rates are due to variations in the actual number of crimes which occur, or to variations in the probability of a criminal being arrested. Until this can be determined, it is impossible to ascertain the effects of changes in legal responses.

[13] It is conventional to differentiate between crude rates, which take as their basic population-at-risk the entire society, and refined rates, which take as their basic population-at-risk only those portions of the society directly at-risk for a particular event. For example, in demography the crude birth rate is the number of births per year per 100,000 members of the population, whereas the refined birth rate is the number of births per year per 100,000 women. A still more refined birth rate would measure fertility in terms of the number of births per year per 100,000 wives of childbearing ages.

Bell notes some interesting pitfalls associated with crude crime rates (1960:153). The United States, like Canada, conducts a decennial census. As a consequence, the crude crime rates per 100,000 population, as offered by the Uniform Crime Reports, suggest distinct cyclical trends. Every ten years, the apparent crime rate drops dramatically. From one census to the next, the crime rate appears to be increasing because the rate is calculated in terms of a hypothetical stable population estimated from census data, whereas in fact the population is increasing. The more rapid the population growth, the greater the distortion. Such errors are increasingly being corrected by application of more sophisticated techniques in estimating annual population changes.

[14] In passing, it might be noted that, under some

circumstances, to be eligible for criminal prosecution, a person must not only reside in the geographic area, but must also be a citizen of the country. Thus, some officials of foreign governments may be granted diplomatic immunity, which renders them ineligible for prosecution for many offenses, usually but not exclusively those of a minor nature.

[15] In the consideration of serious crimes, the legal statutes being violated are often applicable at the federal leval, and so are uniform across the country. For example, in Canada laws involving murder and manslaughter apply uniformly to all parts of Canada, and researchers interested in capital punishment are therefore justified in taking the entire nation as their unit of analysis (Jayewardene, 1972; Teevan, 1972).

[16] In many other instances, uniformity in the law-on-books is achieved only at the provincial or state level. Most studies of deterrence have been based on American data, and have taken a single state as their basic unit of analysis. State crime rates are then used as the basis for between-state and/or within-state comparisons, and variations are considered relevant data for the assessment of the deterrent effect (Sellin, 1959, 1961, 1967a; Gibbs, 1968a; Tittle, 1969; Gray and Martin, 1969; Bailey, Gray and Martin, 1970; Chiricos and Waldo, 1970; Bean and Cushing, 1971; Logan, 1971a).

[17] Some legal statutes are applicable only at the local level. Definitions of minor crimes, such as parking violations, may vary markedly from one municipality to the next.

[18] Some ordinances are applicable only at the city level. Where such is the case, the largest relevant base population for the computation of crime rates is the city as a whole, which must be taken as the basic unit of analysis (Savitz, 1958; Schwartz, 1968).

[19] Teevan (1972) uses as the base line for crime rates only the population seven years of age and older, the age at which, under present Canadian law, a child is eligible to become a delinquent.

[20] In Canada, under the Juvenile Delinquent's Act, the definition of adult status varies from province to province, and

occasionally is different for males and females. The proposed Young Offenders Act suggests that the upper limit for all delinquency should be 17 years of age. At the discretion of the judge, exceptions may be made when the offense in question is very serious (e.g., treason, capital murder, or rape) or when it is deemed in the defendant's own best interests. Such exceptions constitute only a fraction of all juvenile offenses (Cousineau and Veevers, 1972b).

[21] For example, only employed persons are eligible to become embezzlers, and only married persons may be charged with desertion. If one wished to assess the impact of legal sanctions against abortion, the appropriate base line would not be the entire population, but only the women in the fertile ages--at most, those from 10 to 50 years of age. With some exceptions, the suitable base population for charges of driving while impaired would be those persons who have a driver's license.

In the calculation of crime rates, the base population is usually defined in terms of persons who are eligible and likely to commit crimes. For the study of deterrence, however, it is also important to consider some crime rates in terms of those who are likely or eligible to become victims or "objects" of crime. For example, for some offenses, every person in the population is potentially eligible to become a victim. However, for some offenses, only some kinds of persons are eligible to be victimized. Thus, statistics on the incidence of rape could be based on the number of women in the population. Studies of victims of forcible rape suggest that most specifically the population-at-risk might be defined as women over 10 and under 50 years of age (Reckless, 1973:99).

It is also interesting to note that the population-at-risk to become victims may be defined in terms of objects. Thus, Wilkins considers changes in the rate of larceny from motor vehicles in terms of the total number of motor vehicles (cited by Walker, 1971:84). Similarly, McDonald relates traffic and parking offenses to the number of registered motor vehicles (1969:273). Giffen considers the number of convictions for impaired driving in terms of the rate per 100,000 licensed motor vehicles (1965:65).

[22] Some dictionary definitions of these terms may be instructive. According to The American College Dictionary (Barnhart and Stein, 1964), an inclination is "a set or bent (especially of the mind or will), a liking or a preference." A propensity, like a proclivity, is defined as a "natural or habitual inclination or tendency." A predeliction refers to "a prepossession of the mind in favour of something, a partiality." Finally, a predisposition may refer either to simply

"the condition of being predisposed" or, in a sense relevant for the present discussion, a "condition in which a slight exciting cause may produce the crime."

[23]All research concerning criminal activity suggests that the involvement of males in non-legal activities is much greater than that of females. For example, Giffen reports that in 1961 the overall ratio of persons convicted of indictable offenses is eleven males to one female (1965:64). When specific crimes are considered, the direction of difference varied from a ratio of 1.4 to 1 for family offenses to 129 to 1 for sex offenses. Although the ratio of male-female differences is apparently declining, the discrepancy in criminal involvements is still large. For example, in 1967 in Canada, seven times as many men as women were convicted of indictable offenses (Cousineau and Veevers, 1972a:17). Reckless notes that before World War II, the sex ratio of arrests in the United States was as high as 10 to 1 (1973:82). Although it has since declined, to approximately 7.4 to 1 in 1965, in 1970 there were still about six times as many men arrested as women. Women are "under-arrested" for crimes such as robbery, burglary, auto theft, assaults, and handling stolen property. "With the exception of prostitution and commercialized vice, there is no category in which women commit more crimes than men" (Bloch and Geis, 1965:174).

[24]The rate of crime is uniformly reported to be highest in the late teens and early twenties, and thereafter to decline steadily with advancing age. This conclusion holds whether crime is measured by arrests or by convictions (Bloch and Geis, 1965:172; Reckless, 1973:81). Giffen reports conviction rates for indictable offenses for Canada for 1961 (1965). For males, the rates per 100,000 by age were as follows: age 18-19, 1,899; age 20-24, 1,336; age 25-29, 760; age 30-34, 524; age 35-39, 411; age 40-44, 311. In comparison, the highest rate of women, reported for the 18-19 year-olds, was only 119 per 100,000, and the rate for men over the age of 65 was only 55 per 100,000. The concentration of crime among the young varies to some extent with the specific crime under consideration. Thus, Glaser reports that the median age for felony arrests ranged from 16.9 years for auto theft, to 32.8 for embezzlement and fraud (1964:469). However, the overall magnitude and consistency of differences is convincing enough for Bloch and Geis to conclude: "It is generally accepted. . . that crime is largely an act of the relatively young male (1965:174).

[25]In the United States in 1970, approximately 12 per cent of the total population were black: however, approximately

27 per cent of all persons arrested were black. The disproportionate representation of blacks in all kinds of crime statistics is well documented. In addition, "Negroes contribute most disproportionately to serious crimes relating to aggression such as robbery" (Bloch and Geis, 1965:182). In interpreting such conclusions, it is important to remember that the use of base population statistics from the census, which is usually taken as gospel, may in itself be problematic. Siegel (1968) notes, as have others, that the category of persons most likely to be under-estimated in census counts are young urban blacks, the very population which is most likely to be involved in most kinds of serious crimes. To the extent that this is true, it is possible that the over-representation of blacks in serious crimes is exaggerated by the under-representation of blacks in census statistics.

[26] Generally speaking, crime rates are higher in urban areas than in rural areas. In 1970, in the United States,

> . . . the rate for the total seven index crimes in the metropolitan areas was almost twice as high as that in the other urban areas and more than three and one-half times greater than in rural districts (Reckless, 1973:66).

With some exceptions, the same generalizations appear to hold for Canada (Giffen, 1965).

[27] Regional variations are known to account for a substantial proportion of the overall crime rate. Over two decades ago, Porterfield was concerned with regional variation in suicide and homicide, and, on the basis of Vital Statistics, concluded that "the southern states and cities in the main exceed the non-southern states and cities in homicide, but fall behind in suicide" (1949:488). Shannon examines state variations in rates of five index crimes (murder, aggravated assault, robbery, burglary, and larceny) over a period of seven years:

> Since vast differences in crime rates on a sectional basis are found to persist over a period of time, one may hypothesize that subcultural variations of a regional or sectional nature are responsible for these regional or sectional patterns of crime. (1954:273)

Gastil considers regional variations in terms of their degrees of "southerness:"

> Both qualitative historical evidence and multiple

> regressions indicate that the degree of "southerness" in the culture of the population of the states accounts for more of the variation in homicide rates than do other factors such as income, education, per cent urban, or age. (1971:412)

Working at the same time and from the same perspective, Bean and Cushing conclude that

> . . .the simple knowledge of whether a state falls inside or outside the South explains 62.3 per cent of the variation in the criminal homicide among all states. The measures of legal reactions to criminal homicide--severity and certainty of punishment --uniquely account for an additional 7.3 per cent. (1971:287)

Although 7.3 per cent seems like an insubstantial amount, it may account for a considerable part of the variation when other factors are held constant. For example, Bean and Cushing also report that "certainty and severity of punishment account for 19.3 per cent of the variation not accounted for by region" (1971:287).

[28] The significance for crime rates of the three variables, sex, age, and race, can be clearly illustrated with reference to one specific crime, that of homicide. Wolfgang (1958) conducted a study of Patterns in Criminal Homicide in Philadelphia. He reports that the crude rate of homicide from 1948 to 1952 was 6.0 per 100,000. When this rate is refined to consider only persons 15 years of age or older, it is found to be 7.7 Wolfgang reports that the rates of homicide are clearly higher for men than for women, for blacks than for whites, and for persons in the early twenties than for any other age group. The amount of variation in refined rates is striking: white women average 0.5 homicides per 100,000 compared with 4.2 for white man, 12.4 for black women and 57.7 for black men. Considering the additional variable of age, Wolfgang reports the rate for black men aged 20-24 to be 92.5 per 100,000, more than ten times the average. In considering homicide, the population-at-risk most likely to become offenders is to be found among young black men, and it is their offense rate which would be the most significant for the study of deterrent effect of different kinds of legal sanctions.

CHAPTER SIX

REQUIREMENTS FOR MEASURING LEGAL RESPONSES

In studying general deterrence, we can focus on the legal responses as specified by the law-on-the-books or by the law-in-action. However, the consequences specified by law are not always identical to the ones imposed.[1] Further the severity of the law-on-the-books is hard to determine; the gradations of legal responses often leave a range so large that without some form of measurement severity cannot be gauged.[2] In part, deterrence evaluation involves accounting for the certainty, swiftness, and severity of legal responses, but these factors can only be evaluated when the responses actually occur and then can be measured. As a result, responses-in-action are the appropriate indicators for studies of deterrence.

Despite these considerations, several studies of deterrence assess legal responses only in terms of the severity of possible sanctions on the books. This is particularly common in the testing for the deterrent effect of execution. In America, crime rates for states which had legislative provision for death were compared with those which did not have such legislation (Sutherland, 1925; Schuessler, 1952; Bailey, 1973:2). A single state was also compared before and after the abolition or re-introduction of the death penalty (Bedau, 1967; Sutherland and Cressey, 1970; Bailey, 1973:3). These studies, however, do not take into account the possibility that legislative provisions for execution are not the same as conviction rates, which in turn are not the same as execution rates (Barber and Wilson, 1968). It is important to remember Jayewardene's point: execution is a function of the probability of being detected, arrested, charged, tried, convicted, sentenced to death, and of being executed (1972:9). Even drastic changes in legislation may not provide a test of deterrence. Schwartz (1968) uses legislative changes[3] in the penalties for forcible and attempted rape[4] as indicators of changes in legal responses, yet he does not refer to the frequency of imposition.[5] The range of these potential sanctions is so large that, in the absence of information on actual dispositions, severity was not measured. These considerations warrant that law-on-the-books provides a poor measure of legal responses for the evaluation of general deterrence.

Further, law-on-the-books is a poor measure of legal responses because it does not allow for the determination of certainty and swiftness of legal responses. These data represent essential elements in the hypotheses, without them any testing of general deterrence is attenuated. In addition, it may be that increases in the severity of law-on-the-books reduce the certainty and swiftness of laws-in-action and, in turn, decrease deterrent effects. In the Connecticut "Speed Crackdown," legislative changes were introduced to deter speeding by "getting tough" through increased fines, suspension of licenses, and prison sentences (Campbell and Ross, 1968; Glass, 1968). As a result, the discrepancy between law-in-the-books and law-in-action became clearly apparent. Research shows that law enforcers are more reluctant to act when sanctions are "too severe." As a result, there is a reduction in certainty and swiftness of sanctions. Changes in law-on-the-books may indicate changes in law-in-action, but the exact nature of this relationship, as demonstrated in the "Connecticut Crackdown," is difficult to establish.

LEGAL RESPONSES FOR INITIAL VERSUS RECIDIVIST OFFENDERS

As discussed in the previous chapter, it seems reasonable to argue that a crime rate composed of initial offenders be used as an indicator of the relationship between legal response and crime. In addition, legal scholars defend the idea that legal responses for initial offenders should differ from those for others. Available data appear to support this theory. These findings suggest that the legal response index should consist of data on the legal responses for initial offenders. Without arbitrarily ruling out other possibilities, we will proceed to examine some arguments in favour of this position.

It has been stated that would-be offenders are more likely to be affected by legal responses designed for people resembling themselves (Nettler, 1974a:33). It is judicial custom to consider the offender's previous record prior to imposing a legal response. It is also generally believed that initial offenders should receive legal sanctions different from those given to recidivists.[6] As a result, initial offenders are less likely to be incarcerated,[7] and more likely to serve shorter sentences, than are recidivists.[8] There is evidence that the certainty of conviction due to plea bargaining[9] and the subsequent length of sentence are different for initial offenders than for recidivists.[10] These findings indicate that the legal responses imposed on initial offenders are different from those imposed on recidivists. Yet no assessment to date has used legal response to initial offenders as an index of deterrence.[11]

SWIFTNESS OF LEGAL RESPONSES

The concept of celerity refers to the time involved from the commission of an offense to the administration of the legal sanction. It is applicable to all stages of legal responses up to conviction and the imposition of sanctions.[12] Perhaps celerity is best expressed as a cumulative index summarizing the time taken between the commission of a crime through arrest to disposition. Presumably, it can also be applied to intermediate steps, yet there have not been any studies of deterrence that have involved the swiftness of legal responses.[13]

CERTAINTY OF LEGAL RESPONSES

Certainty of legal responses involves a chain of events consisting of the likelihood that an offender will be detected, arrested, charged, tried, and convicted. Certainty can be ascribed to both offenses and the offenders[14] but because general deterrence concerns the latter, the certainty of responses to offenders provides an appropriate indicator for the evaluation of deterrence.

Unfortunately, most research does not provide an appropriate measurement of certainty. Most studies use an index comprised of a denominator representing the number of crimes known to the police, and a numerator representing the number of persons known to have experienced a particular kind of sanction. For example, Gibbs (1968a) measures certainty by a ratio expressed as the number of persons admitted to prison for the crime of homicide over the number of homicides known to the police. Tittle (1969) gauges certainty as the ratio of the number of persons admitted to prison over the number of crimes known to the police. Similar indices are formulated by Bailey, Gray, and Martin (1970), Chiricos and Waldo (1970), Logan (1970), Sjoquist (1970), Ehrlich (1972, 1973), and Orsagh (1973).

In all these cases, however, a failure to differentiate certainty in terms of offenses from certainty in terms of offenders introduces a degree of unreliability. These studies do not account for such possibilities as one offender having committed a number of offenses, or conversely, the involvement of several offenders in one offense. For example, if the number of homicides known to the police is compared with the number of persons eventually executed, the certainty index may be distorted by a multiple murderer, who can be executed only once. In the U.S., if murder is combined with suicide, the crime may never be "cleared by conviction," while in Canada, it would be lost in the heterogeneous category of "cleared otherwise." At the other extreme, it is possible to convict more than one person for a single murder. Thus Tittle points out that certainty indices with a denominator of offenders and a numerator of offenses reflect the probability that offenses rather than offenders will be sanctioned (1969:412).

Other researchers have used ratios made up of units other than admissions to prison and crimes known to the police. One method divides the number of crimes cleared by arrest by the number of crimes known to the police (Teevan, 1972; Votery and Phillips, 1972; Phillips and Votery, 1972; Logan, 1974; Rowe and Tittle, 1974). Teevan (1972) uses a summary index of certainty with a numerator of the number of convictions and a denominator of crimes known to the police. Other indices used include the number of offenses "cleared" divided by the number of crimes known to the police (Carr-Hill and Stern, 1973) and the number of cases cleared by arrest divided by the number of policemen on the force (Tittle and Rowe, 1974:7). The research to date has thus failed to provide an appropriate measure of certainty. Indices which focus on offenses do not contribute any data about offenders representing the primary units of analysis in the study of general deterrence.

SEVERITY OF LEGAL RESPONSES

The element of severity of legal responses has been applied mainly to sanctions. This discussion, therefore, focuses on that aspect of severity, although the possibility of severity in inculpation and adjudication will be considered briefly.

There are several kinds of legal consequences, for example, those which: (a) restrain mobility and personal freedom,

(b) deprive the individual of his property, (c) enforce labour, or (d) terminate the offender's life (Bloch and Geis, 1954:34; Zimring and Hawkins, 1973:173-194). These consequences may be achieved by legal sanctions such as suspended sentence, probation, fines, restitution, capital and corporal punishment, sterilization and castration, and various forms of imprisonment (Tappan, 1960:421-436). Deterrence studies, however, have focused on only two kinds of sanctions: capital punishment and imprisonment.

Severity and Death Penalty

Considerations about the severity of execution results in several views. Becarria opposes the death penalty because he thinks it is less severe than life imprisonment (Manzoni, 1964:48). Goyer also raises the question of whether or not life imprisonment is more severe than execution (1972:86). Tullock argues that life imprisonment is frequently considered more severe than execution, even in the eyes of the potential murderer (1974:104). It is also worthwhile to note that the severity of execution has been subject to some legal contention (Long, 1973:219).

Regardless of these considerations, contemporary students of deterrence consider execution to be the most severe sanction. Most researchers assume that the severity of execution is responsible for its status as a possible deterrent (Sutherland, 1925; Schuessler, 1952; Savitz, 1958; Sellin, 1967; Bedau, 1967; Bailey, 1973).

Questions have been raised about the severity of execution, but the relative severity of actual or believed methods of execution appears to have been overlooked. Death by gas, electricity, or hanging may constitute degrees of severity. It has been suggested that the method of execution be related to the kind of deterrence expected. If the aim is specific deterrence, then the method of execution does not matter, but if the goal is general deterrence, "the aim is to make the offenders' end a terrible warning to others; and if so, it should presumably be made painful, ignominious, and spectacular" (Moberly, 1968:271).

Severity and Imprisonment

The other legal sanction commonly examined in studies of deterrence is imprisonment. With regard to imprisonment, severity is considered to be proportional to the length of time served in prison (Gibbs, 1968a; Tittle, 1969; Gray and Martin, 1969; Chiricos and Waldo, 1970; Bean and Cushing, 1971; Logan, 1972; Jayewardene, 1972; Bowers and Salem, 1972; Teevan, 1972; Ehrlich, 1972; Votery and Phillips, 1972; Phillips and Votery, 1972; Carr-Hill and Stern, 1972; Ehrlich, 1973; and Orsagh, 1973).[15] The emphasis on the severity of imprisonment results in certain limitations for assessing deterrence primarily because it is an atypical sanction.[16]

THE PROBLEMS OF DEFINING SEVERITY

Researchers appear to have avoided defining and measuring severity vis-a-vis both these sanctions--capital punishment and imprisonment. Observing this problem, Green concludes that

> . . .the various types of penalties are incommensurable in terms of some common unit of measurement. Although we can say that one type of penalty is more severe than another, it would be impossible to state objectively the length of term of probation or the amount of fine that would equate in punative power, deterrent effect or rehabilitive value with a given period of imprisonment. (1961: 26)

In order to assess the impact of legal sanctions on crime, and to make comparisons within and between sanctions, we must first attempt to define and measure severity.

The measurement of severity raises several questions. The first relates to the dimensionality of the concept: is severity a unidimensional or a multidimensional variable? In other words, can measurement of severity allow an assessment of the impact of amounts of sanctions, the relative impact of different kinds of sanctions, and the impact of combined sanctions? Second, what authority is competent to assign degrees of severity to various sanctions for a scaled measurement? Third, does the concept of severity apply only to legal sanctions, or does it include other stages of legal responses?

The Dimensionality of Severity

There is no agreement on the dimensions of severity. Green considers it to be a unidimensional concept best measured in degrees of deprivation of civil liberties (1961:23). Zimring and Hawkins suggest a multidimensional notion based on "types of unpleasantness" (1973:193). They include economic deprivation, loss of privileges, confinement, pain, death, and stigmatization as different dimensions of severity. Unlike Green, these authors are not concerned with the issue of dimensionality per se; they use the word as if it was a unidimensional concept (Zimring and Hawkins, 1973:194-197).

It is reasonable to assume that severity is a multidimensional phenomenon reflected both within and between types of sanctions. For example, given the differences within and between prisons, length of sentence represents only a partial measure of the severity of imprisonment. Furthermore, to equate length of time in prison with length of time on parole or probation would be specious. Difficult as it is, the task of measuring and defining severity is important to the evaluation of deterrence.

Mean Versus Median Prison Terms

Most studies judge severity either in terms of the average time served in prison (Tittle, 1969; Bailey, Gray and Martin, 1970; Sjoquist, 1970; Ehrlich, 1972) or by the median time served (Gibbs, 1968a; Tittle, 1969; Chiricos and Waldo, 1970; Teevan, 1972). Tittle (1969) and Schwartz (1968) also refer to the maximum length of sentences allowable by law.

The use of both the mean and the median as a measure of severity may be questioned on the grounds that these data are "artificial" and do not represent the actual facts. The median is a convenient but unnatural breaking point in the data, which has been arbitrarily established. This criticism is supported by the fact that there is no justification for the choice of the fiftieth percentile as the cutting point; it would be equally logical to propose a higher percentile, such as the seventieth. On the other hand, the mean can also be criticized as representing an artifact, one which may be particularly misleading if there is a large spread in sentencing patterns.[17]

Modal and Exemplary Sentences

Other reasonable indicators of severity include modal sanctions. These are the sanctions occurring most frequently[18] and represent in a certain sense a "natural" clustering of data. The second measure which may be adopted is the grouping of the longest sentences imposed. When a particular kind of offense seems to be increasing in frequency, courts may impose sanctions that are longer than usual (Walker, 1969:68; Zimring and Hawkins, 1973:46). These "exemplary sentences" are intended to provide greater deterrence than the typical sanctions.

It appears then that the length of time of imprisonment can be judged by several measures. However, general deterrence seems just as adequately tested by applying extreme or modal measures of imprisonment. But even if these measurements become accepted, we are still faced with the utilization of measurements to determine the severity of serial and combined sanctions.

Multiple and Serial Sanctions

Researchers of deterrence have not concerned themselves with multiple sanctions, such as a combination of fines and imprisonment, or imprisonment and corporal punishment, for an offense. It is not clear how corporal punishment--McGrath estimates that between 15 and 35 Canadians are lashed each year (1965:5)--can be compared to other sanctions, or how combined sanctions can be compared to single sanctions.

Serial sanctions are also used, especially in the form of imprisonment followed by parole (Cousineau and Veevers, 1972a). Serial sanctions should be taken into account for studies of severity. Unfortunately, there remains the problem of assessing the relative impact of different kinds of sanctions. This task requires a scale for severity.

Scaling

No attempts appear to have been made to test deterrence by the use of a scale for legal responses. Since the scaling of severity entails judgments,[19] selection of the scaling methodology[20] and, of the respondents whose judgments are to be scaled, may be a problem.[21] Such a selection would depend on the conception of deterrence to be tested. If one considers the entire adult population to be potentially and equally at risk, its opinion on severity would be important. If, on the other hand, one considers only a marginal group to have a proclivity to crime, then that group's perceptions of legal responses would be relevant.

In addition to the entire population and a marginal group, other groups of judges may be representative. Severity might be judged by the agents of legal responses, that is, judges, magistrates, and juries, as well as those involves in the implementation of sanctions, such as wardens, police, probation and parole officials. However, no matter what segment of the population is chosen to judge legal responses, some *a priori* concept of the population-at-risk is implied, and any resulting scale would be open to debate.

All research on deterrence *assumes* some hierarchy of severity within and between sanctions. Such assumptions may be reasonable, but they are not synonymous with measurement. It seems clear that severity is not only a multidimensional concept, but that it varies with different kinds of sanctions. There are many problems in comparing the severity within and between sanctions for different categories of potential offenders. If the severity of one kind of sanction differs from that of some other sanction, then these varying types of severity cannot be added together to obtain the effect of multiple or serial sanctions. The possible multidimensional nature of severity within a sanction type adds additional difficulties to the assessment of deterrence.

Severity of Inculpation and Adjudication

Researchers of deterrence tend to assume that severity applies only to sanctions. However, arrest, for example, has been considered a sanction as well as a step in inculpation (Rowe and Tittle, 1974; Logan, 1974). In 1968 in England, Willcock and Stockes surveyed a random sample of 808 men aged 15 to 22, most of whom had had no official contact with law enforcement agencies (cited by Walker, 1969:63-68). The men were shown, in random order, eight cards each describing a possible consequence of detection of crime and were asked: "Which of these things worry you most about being found out by the police?"

Nearly half (49 per cent) were most concerned about "what my family would think about it," and over a fifth (22 per cent) were most worried about the possibility of losing their jobs. The next most common response (12 per cent) concerned the "publicity" or shame of appearing in court; only 10 per cent indicated worry about the expected punishment. Similarly, in discussing arrest, McGrath suggests that

> . . .for a well-established member of the community, the real punishment would be discovery, and public disgrace for himself and his family. If this consideration will not deter him, neither will fear of an uncertain sentence following possible conviction. The risk of prison is part of the way of life of the professional criminal. His concern, too, is with not getting caught. (1965:8)

McGrath implies, but does not pursue, an important point, namely, that the impact of the processes of arrest and trial on potential initial offenders may be different from the impact on potential recidivists.

It has been suggested that arrest may have a stigmatizing effect, which may concern potential offenders as much as do subsequent legal responses (Zimring and Hawkins, 1973:183-190). In a unique study, Schwartz and Skolnick examine the effects of a criminal court record on the employment opportunities of unskilled workers (1964:104-110). They found that

> . . .the individual accused but acquitted of assault has almost as much trouble finding even an unskilled job as the one who was not only accused of the same offense, but also convicted. From a theoretical point of view, this result indicates that permanent lowering of status is not limited to those explicitly singled out by being convicted of a crime. (1964:108)

The nature of the offense as well as the extent of its publicity may affect the degree of stigmatization. Degrees of stigmatization could be regarded in terms of severity.

The physical danger which may be involved in arrest may also be considered as a form of severity.[22] Westley suggests that police departments may have a "morality of secrecy and violence" which justifies the use of force, especially with regards to offenders "known to be hardened criminals" (1970: 121-122).[23] In short, the presence or degree of violence at arrest may be considered a form of the severity of inculpation.

Inculpation may also be assessed for severity in terms of the availability or amount of bail, and the length of pre-trial detention.[24] Bail and pre-trial detention have been found to be related to the outcome at later stages in the administration of justice.[25]

In conclusion, deterrence literature contains various problems in regards to the scope and applicability of the concept of severity. If severity is related to legal sanctions, the concept of dimensionality is less complex than if severity is also considered in terms of inculpation. If the concept is extended to all stages of the criminal justice system, severity becomes multidimensional. As such, severity may be too heterogeneous a variable to categorize and measure within a single ruberic.

To date, students of deterrence have not devised an adequate measure of legal responses. In addition, they have rarely been aware of the problem of contaminated indices, or more specifically, the use of indices which are comprised of both measures of the crime rate and legal response indicators.

JANUS-FAÇED INDICATORS

Testing deterrence hypotheses requires that the measurement of legal responses be independent from the measures of criminal behaviours. Chapter Five demonstrates that offenses known to the police do not constitute a valid or reliable index for testing general deterrence. We conclude that the "best" crime rate would be the one comprised of arrested initial offenders. In this chapter we note that arrest is also used as a measure of legal response. The assessment of general deterrence is therefore hindered by the failure to use independent measures of crime and legal responses. For example, Orsagh (1973) gauges certainty by the number of convictions (a part of his independent variable, legal responses) per crimes reported to the police (his dependent variable). This type of error is also made by Sjoquist (1970), Phillips and Votery (1972), Votery and Phillips (1972), Carr-Hill and Stern (1973), and Ehrlich (1973).

Such indices result in increases in the correlation between crime rates and legal responses, which favour the deterrent hypotheses, and as such are referred to as indexical artifacts. These phenomena will be considered further in the discussion of spuriousness in Chapter Nine. To the extent that valid and reliable measures of crime and legal responses pertinent to testing and evaluation are not possible, the confirmation or refutation of general deterrence is vitiated.

We now turn to what is probably the most crucial issue in the evaluation of general deterrence; the possibility of specifying and satisfying some criteria for attributing causality to the proposed relationship between legal responses and crime rate.

FOOTNOTES CHAPTER SIX

[1] This discrepancy is most apparent in studies of the death penalty where the legal provision for execution is not the same as the number of executions (Barber and Wilson, 1968).

[2] Schwartz (1968:509) reports that in Pennsylvania the penalty for rape ranged from a minimum of 15 years imprisonment to a maximum of life imprisonment--a range of as much as 40 years. For other offenses, the sanction may not be stipulated, or may be phrased so generally as to be impossible to scale. Sellin and Wolfgang (1964) scale legal sanctions in terms of the maximum prison sentences for various crimes in the Penal Code. However, this measure is truncated at its lower end, in that the smallest maximum penalty is 30 days in gaol. At the opposite extreme, the researchers are forced to estimate the penalty involved in "life imprisonment" in terms of the offender's age at the time of sentence and his life expectancy. To these limitations must be added the fact that the maximum penalties of the Penal Code provide relatively few intervals between 30 days of imprisonment and life imprisonment (Stevens, 1968:191).

[3] These changes were precipitated by a series of particularly offensive crimes. Schwartz reports:

> On April 3rd, 1966. . . .three Negro men broke into a West Philadelphia home occupied by an eighty-year-old widow, her forty-four-year-old daughter and fourteen-year-old granddaughter. . . the intruders viciously beat up and raped both women and the child, ransacked and looted the home. . .Each of the three victims was ferociously dragged and thrown about. . . the upstairs and downstairs were spattered with blood. The grandmother later died of her wounds. (1968:509)

The degree of atrocity associated with these crimes led to an exceptionally intense public outcry, voiced and fanned by the coverage given them by The Philadelphia Inquirer. "By the middle of April the Palm Sunday Rape in West Philadelphia had become a cause celebre throughout the state" (Schwartz, 1968: 109). The state legislature devoted several special sessions to considerations of increases in penalties for rapists, and within two weeks the Pennsylvania Penal Code of 1939 had been amended, with dramatic increases in prescribed legal penalties for rape.

[4] For cases with bodily injury, the maximum sentence for rape was increased from 15 to 20 years imprisonment, and the maximum sentence for attempted rape was increased from 5 to 7 years imprisonment. For cases with bodily injury, the maximum sentence for rape was increased from 15 years to life imprisonment, and the maximum sentence for attempted rape was increased from 5 to 15 years imprisonment. For persons convicted more than once for rape or attempted rape, the maximum sentence was increased from 5 years to life imprisonment; when considering inveterate offenders, no distinction appears to be made between cases involving or not involving bodily injury. For a complete discussion of these legislative changes, see Purdon's Pennsylvania Legislative Service (State of Pennsylvania, 1966:27-28).

[5] Schwartz's study makes the implicit assumption that the drastic changes in the potential legal penalties for rape are directly associated with comparable changes in the actual legal penalties for rape. There is, however, reason to assume that this might not be the case. We have no indication of the frequency with which the courts actually have imposed the $10,000 fine or have sentenced offenders to life imprisonment, nor do we know the frequency with which, when such severe sanctions were indicated they were actually imposed on offenders. There is reason to believe that the drastic increase in the severity of sanctions might be associated with an equally drastic decrease in the certainty of sanctions, incorporating a host of factors from the reluctance of the woman to press charges, to plea-bargaining, to the reluctance of the courts to return verdicts of guilty.

[6] This belief is so widespread as to be largely taken for granted. For example, Gibbons asked respondents to indicate the relative seriousness of 20 criminal acts, and to indicate the sanctions they felt would be appropriate (1969:394). In most, but not all, cases he believed it relevant to tell his respondents the criminal record of the offender described: thus, some crimes were described as committed by persons without prior criminal records; some were described as committed by persons with previous convictions for other kinds of offenses; and some were described as committed by persons who were inveterate offenders. Unfortunately (for the purposes of his study), the attempt to isolate the relative seriousness of different kinds of crimes is obscured by the fact that the crimes are inconsistently described as being committed by first offenders and by known criminals. The resulting apparent differences in seriousness may reflect the common belief that crimes committed by recidivists are more serious and deserve harsher punishment than similar crimes committed by otherwise honest citizens.

[7] Walker (1971:49) notes that, holding constant the specific offense, 18 per cent of first offenders for violent crimes were sent to prison, compared with 43 per cent of those with previous convictions of non-violent crimes and 72 per cent of those with previous convictions of violent crimes. Similarly, Green reports that of persons convicted of felonies, only 70 per cent of first offenders were sent to prison, compared with 82 per cent of persons with one previous felony conviction and over 90 per cent of persons with four or more previous felony convictions (1961:115).

[8] Green differentiates between previous felonies and previous misdemeanours observing that

> . . .only in cases involving no prior felony convictions does variation in the number of prior misdemeanour convictions significantly affect the sentencing of the court. . . .The number of prior convictions for felonies, however, exerts a strikingly significant effect upon variation in the severity of the sentences; the percentage of penitentiary sentences in each of the categories is as follows: non, 14.4; one, 27; two, 35.5; and four or more, 50.7. (1961:44)

In the same vein, Devlin concludes that

> . . .there is no doubt that the penal record of the offender is of the greatest significance in assessing the sentence and is evidenced by the number of statutes which call for higher sentences in respect to persons with a history of previous convictions. (1970:46)

[9] Recidivists are assumed to know more than do first offenders about pleading guilty for considerations (Carney and Fuller, 1969). They believe that such a procedure will result in a lighter sentence than would otherwise be imposed, and they are sophisticated in arranging it. In Newman's words, they are "conviction-wise" compared with the relatively naive first offender (1956:784). In the second place, recidivists have more reason to try to "cop a plea," in that their records increase the probability of conviction and severe sentence, especially with a jury trial. Newman observes that they are also "conviction-prone" compared with the first offender who has an otherwise unblemished record (1956:784).

[10] There is some evidence that persons who plead guilty tend to draw shorter sentences than do persons who stand trial.

The study of plea bargaining suggests that differences in the sanctions are most apparent when comparing first offenders who plead guilty with recidivists who plead guilty. In fact, in those instances where first offenders elect to stand trial, they may actually receive longer sentences than recidivists who were "conviction-wise" enough to forego their right to trial in favour of a shorter sentence. Discussing this point, Carney and Fuller note:

> The "inducement" aspect of plea bargaining, i.e., the pressure on the defendent to enter a plea of guilty was clearly illustrated in the case of United States versus Wiley. In this case, the judge imposed a heavier sentence on one of the five defendents explicitly because he refused to plead guilty. (1969:295)

[11] Given the fact that first offenders are less likely to be incarcerated than other offenders and, if incarcerated, are likely to be given shorter terms, it is not surprising that the number of first offenders imprisoned is disproportionately low. Morris reports: "First offenders. . .constitute a minority type of penitentiary inmates. Probably three-fifths or more of penitentiary prisoners are those who have previous records of criminal activity" (1941:139). In Canada, in 1969, of all persons admitted to penitentiaries, 79 per cent had been previously incarcerated (Cousineau and Veevers, 1972a:15). The disproportionate representation of recidivists in prisons suggests a rather serious flaw in some research ostensibly concerned with general deterrence. Researchers who consider the severity of sanctions in terms of the mean number of months of prison terms sentences or experienced are, in fact, considering the severity of sanctions imposed most on known criminals, rather than on first (or initial) offenders.

[12] The President's Commission notes that the speed with which felony cases are processed shows substantial variations depending upon factors other than the law itself and the nature of the purported crime. The Commission suggests a time table for the processing of felony cases as follows:

1. Arrest to first judicial appearance: within 24 hours.
2. First judicial appearance to formal charge (indictment or filing for the information): within 72 hours for incarcerated defendents and 7 days for released defendents.
3. Formal charge to pretrial proceedings: within 19 days of arraignment on the indictment and entry of plea.

> 4. Pretrial to trial: barring exceptional circumstances, within 9 weeks of arraignment.
> 5. Conviction to sentencing: within 14 to 21 days.
> 6. Sentencing to appellate review: within 5 months.
>
> (1967b:84-87)

[13] Wolfgang (1958) takes note of what he calls the "tempo of justice," as measured in 30-day intervals of time in the processing of persons accused of homicide. He does not, however, relate these factors to the deterrence hypothesis.

[14] Considerable confusion is generated by the different measures of certainty based upon offenses and offenders. For example, Claster assesses the perceptions of certainty of delinquents and non-delinquents:

> Each item consists of a definition of one class of criminal offense, an example of that offense, and a question requiring respondents to check one of four percentage figures, at ten per cent intervals, which they believe correctly represents the "cleared by arrest" rates for that crime. The first item is: murder--killing a person on purpose. For instance; a man plans to kill his wife. He buys a gun, takes it home, and shoots her. What per cent of murders end up with someone arrested for the crime? Sixty-two per cent, 72 per cent, 82 per cent, 92 per cent? (1967:81)

From this example it is unclear whether respondents are being asked to estimate the number of offenses or the number of offenders.

[15] The literature on imprisonment assumes that increases in time served are measures of severity. This assumption may be reasonable but few authors examine the potential plateau effects. Jeffery speculates about such a possibility by extrapolating from research in learning (1965:298). He suggests that when severity reaches certain levels, a "satiation" effect occurs. Thus, it is possible that a prison sentence of 25 years is not, in fact, 25 per cent more severe than a prison sentence of "only" 20 years. Although the units involved appear to be equal, from the point of view of deterrence there is reason to suspect that there may be a point of diminishing returns, after which increasing the sanction does not produce proportionate increases in the deterrent effect.

[16] In Canada, in 1967, of all persons convicted of indictable

offenses, 40 per cent were imprisoned (Cousineau and Veevers, 1972a:11). Zimring and Hawkins claim that of the total number of crimes committed, only one per cent resulted in prison sentences (1973:336). Prison populations are also non-representative in that they are comprised primarily of recidivists. First offenders are a minority group, with three-fifths of prisoners having previous convictions (Morris, 1941:139). In Canada in 1969, of all persons admitted to penitentiaries, 79 per cent had been previously incarcerated (Cousineau and Veevers, 1972a:15).

[17] Walker draws attention to a similar distinction when he considers one common measure of legal responses to crime, namely, the "average length of prison sentence" imposed by the courts (1971:52-55). He illustrates the simple point that two instances of an average length of sentence of, say, six years may be alternatively composed of an even distribution of cases over the range of imprisonment from one to twelve years, or the uneven distribution of a cluster of cases of less than two years and another cluster of more than ten years.

[18] To my knowledge, no researcher on general deterrence has made this distinction. One exception deserves special mention, although it is beyond my direct concern in that it focuses upon juvenile offenders. Hagedorn comes close to my meaning of mode when he attempts to measure the uniformity of sanctions, which he defines as "the degree to which the same punishment is applied to all juveniles committing the same offense" (1967:381). Uniformity of sanctions is, then, treated as a separate variable from severity of sanctions.

[19] Most efforts to determine the severity of different kinds of legal responses have subjects rank-order the sanctions. No attempts appear to have been made to scale severity per se. For example, Rose and Prell's (1955) undergraduate students consider one year in prison to be roughly equivalent to a fine of $2,500. Gibbons (1969) studied the public's view of legal sanctions for adults and offers a scale of severity with the following range: execution, prison for over five years, prison for one to five years, gaol for six months, gaol for one month, probation, and a fine of $100. Boydell and Grindstaff (1971) follow a similar procedure with sanctions ranging from execution to a fine of an unspecified amount, suggesting that a fine of any size is less severe than probation for any period of time, which is, in turn, less severe than any form of incarceration.

[20] Hagedorn uses a Thurstone scale to obtain the following

rank-order of sanctions for juveniles: no action taken, no further action taken, unofficial probation, official probation, suspended sentence, commitment to a foster home, commitment to an institution, and commitment to reform school (1967: 383). Terry (1967) rank-orders ten kinds of sanctions for juveniles. In ascending order these are: official responses of police (release, referral to a social or welfare agency, referral to state department of public welfare); official responses of the probation department (release, informal supervision, referral to court, waiver to adult court); and responses by the court (formal supervision and institutionalization). To date, no one has applied a scaling technique like that devised by Sellin and Wolfgang (1964) for measuring severity of legal responses.

[21] Sellin and Wolfgang (1964:249) used an amalgamation of judges to determine the gravity of offenses: policemen, judges, university students, citizens selected for jury duty, and citizens-at-large.

[22] Westley reports that among the police officers he interviewed, 12 per cent felt force was justified "to make an arrest," especially in cases where the person was "known to be guilty" or "known to be a hardened criminal" (1970:121-123). It seems likely that arrest will be more severe for persons who are apprehended in the act than those who are apprehended much later. Catching an offender "red-handed" may tend to increase the probability of police violence of brutality, in that the police have in their own minds the assurance that the accused offender is an actual offender. Conversely, the person who is picked up routinely may elicit less hostility. Westley's data also suggest that the proclivity of police officers to use violence is increased by their observation of persons whom they consider to be "good pinches," because they were "clearly guilty," but who had subsequently received reduced or no sanctions because the court procedures had been "fixed," allowing clever legal tactics to by-pass the spirit of the law. The more a police officer observes his "good pinches" being "fixed" in the courts, the more tempted he is to take the law into his own hands.

[23] Although first offenders are obviously not known by the police as criminals, they may exhibit other traits which may elicit violence in the process of being arrested for the first time. Westley's data also indicate that a significantly high proportion of police felt justified in using force when the suspect did not show sufficient respect for the police (1970: 76-86). In some instances, being arrested may be an especially hazardous experience for the first offender, who does not know

what to expect, and who, therefore, may inadvertently antagonize the arresting officer by exhibiting an inappropriate manner and demeanour.

[24] Thus, The President's Commission notes:

> Pretrial detention involves serious costs for the defendant. The most obvious cost is imprisonment itself, which is particularly harsh and unjust for the accused when conviction does not result in imprisonment, as is often the case. In the Federal System in 1963 approximately 22,340 persons were detained before trial, but only 13,600 were later sentenced to prison. (1967b:38)

[25] The President's Commission remarks on

> . . .the possibility that the outcome of a case will be influenced by the defendant's detention. . . Recent studies tend to confirm the view that pretrial detention increases the likelihood of conviction. The limitations imposed by confinement hamper preparation of the defense. . .Some of the same studies indicate a correlation between pretrial detention and the imposition of a jail sentence rather than probation after conviction.

CHAPTER SEVEN

CAUSALITY AND GENERAL DETERRENCE

Conceptions of general deterrence entail causality. At the broadest level, deterrence assumes a causal relationship between legal responses and criminal activity. As Bowers points out, "the concept of deterrence refers to the causal impact that punishment is supposed to have on deviant or criminal behaviours" (1972:4). Causality is a basic attribute of legal responses:

> When is behaviour controlled? The legal order is a social technique for causing men to behave in ways held to be desirable by those who make the laws. The sanctions that can be imposed by constituted authority may coerce and therefore control men. When coercion causes someone to behave in a certain way, then he is not acting as he chooses, but under compulsion, as someone else chooses. (Brodbeck, 1968:673; emphasis mine)

Zimring and Hawkins point out that the term deterrence is used restrictively, "applying only to cases where a threat causes individuals who would have committed the threatened behaviour to refrain from doing so" (1973:71).

Although the actual term "cause" is not common in deterrence literature, its synonyms are. For example, causation underlies notions such as "coercive power" (Hawkins, 1971:169) and "social control by coercive means" (Gerber and McAnany, 1972:9).

Despite its importance for beliefs about deterrence, causality has been ignored and left implicit. Fundamental questions, such as the attributes of causality, the criteria necessary for establishing causality, and the evaluation of methods for causal analysis, have not been adequately examined. It is to these issues that we now turn.

THE MEANING OF CAUSALITY

The issue of causality is one of the fundamental concerns in the history of Western thought. It would be impossible to trace all its shifts and implications in the various stages of science and philosophy. Fortunately, this study is only concerned with causality's role in the context of deterrence hypotheses. A general agreement does exist about causality's legal employment (Hart and Honore, 1959:25), and social science as well as common usages of the notion do not drift very far from its generic meaning (Nagel, 1965:17).

Causality has two etymological meanings (Macdonald, 1966: 95). The first, which derives from Greek and Roman usage, refers to deeds for which one can be held accountable and for which there are legal responses (Nagel, 1965:17). Used in this way, the term involves the idea of accusation or guilt (Dray, 1964:48). As Nettler (1970:144) and Sjoberg and Nett (1968:286) point out, Western legal and religious systems maintain that "holding persons responsible" involves their being judged as a "cause" of action. In fact, both the terminology and judicial thought of Western legal systems are dominated by causal concepts (Hart and Honore, 1959:5).

The second etymological meaning derives from the adaptations and transformations of the concept for scientific purposes in the Renaissance. Used to refer to external, transpersonal forces, causality acquired its second meaning: necessary production or "that which produces an effect" (English and English, 1958:77).

Both these meanings are so pervasive that they constitute a mental habit (Dahl, 1965:89). The depth to which causality is embedded in Western thought is reflected by the fact that it is variously considered as a part of perception (Michotte, 1963; Arnheim, 1969:279), as a psychological attribute in itself (Lana, 1969:44), as a function of language (Bruner, 1966:43), and as an element of culture (Simon, 1965:157; Nettler, 1970:143).

As with many issues, however, pervasiveness does not constitute sufficient grounds for acceptance, and attempts to reject or at least modify notions of causality have been common. Some writers have suggested that causality be considered a useful assumption (Blalock, 1961:6), while others favour its abandonment (Lerner, 1965:5). Causality has been regarded as a guiding principle (Pap, 1962:311), as a working rule (Simon, 1965:158), and as predictability according to rules (Feigl, 1953:408).

Yet those who have attempted to abandon or modify the term, have not been successful in escaping its synonyms (Scriven, 1968). For our purpose, it may be useful to recall that MacIver (1942) cites a compendium of sociological concepts, all of which entailed the notion of causality. He concludes that the denial of causality would result not only in the denial of concepts such as time, change, and continuity, but also in the successive denial of all concepts (MacIver, 1942:5). Most considerations of causality assign it, in different ways, to events in general as well as to the actions of people (Kelley, 1955; Heider, 1958; Hart and Honore, 1959; Wrightsman, 1972:454; Jones and Aronson, 1973:415).

GENERAL CRITERIA FOR CAUSAL ATTRIBUTION

Causality is considered the sine qua non of scientific investigation (Simon, 1965:158) and represents the central postulate of all science (Eubank, 1932:207). However, criteria for its recognition have met with little agreement.

There are a number of reasons for this dilemma. For example, even though perception in terms of causes may be a mental habit, causal models which are more purposefully employed remain open, and are influenced by moral and personal considerations. Consequently, the criteria by which causes can be recognized have been variably and indiscriminately used, questioned, or abandoned (Nettler, 1970:146). We will outline briefly ten of these criteria, before examining them in detail.

Externality--Causes and effects have ontological status as phenomena existing in the "real," "external" world. This criterion was proposed by Aristotle and Galileo (Bunge, 1959: 174) and was attacked by Hume (1740:116).

Productiveness--Causes produce effects (Aristotle, 1930: 194; Bunge, 1959:46; Madden, 1960:202).

Association--Causes and effects occur together (Hume, 1740:116). Association takes place when at least the following three conditions are satisfied:

Conditional Association--If the cause occurs, then so does the effect (Braithwaite, 1953:314; Bunge, 1959:36).

Invariable Association--If the cause occurs, then the effect always occurs (Galileo, 1623:265; Nagel, 1961:74).

Non-Spurious Association--For the association to be called a causal one, it must be genuine and not due to other factors (Simon, 1957:37; Rosenberg, 1968:24; Nettler, 1970:154).

Sequence--Causes precede effects in time (Russell, 1912: 175; Alker, 1968a:211; Nagel, 1961:74).

Contiguity--Causes and effects occur in spatial proximity (Bunge, 1959:58; Nagel, 1961:74).

Asymmetry--Causes are unidirectional and irreversible in their influence (Simon, 1968:353; Alker, 1968a:211).

Unique--One cause produces one effect (Aristotle, 1930: 194; Nagel, 1965:19; Selltiz et al., 1959:80).

Necessity--Causes are what must be there for the effects to occur (Galileo, 1623:265; Selltiz et al., 1961:81; Nettler, 1970:147).

Sufficiency--Causes are what are "enough" to account for the effects (Susser, 1973:46; Scriven, 1966:259; Zetterberg, 1965:71).

Contingency--Causes depend upon additional phenomena to help them produce effects (Nagel, 1965:15; Zetterberg, 1965: 71; Lerner, 1965:30).

We now turn to a more comprehensive discussion of these criteria.

Externality

Causality was seen as belonging to the phenomenal world, and as such, was believed to have ontological status as an external phenomenon of interdependence occurring among both social and natural events (Bunge, 1959:174). Aristotle, as well as Galileo, called this "efficient" causality because it was an external determinant (Bunge, 1959:174). Epistemological conceptions of causality, on the other hand, held that causation was not an external entity but an idea about the relationship between events (Locke, 1690; Kant, 1781).

Hume (1740:116) attempted to bridge the gap between the phenomenal and the epistemological conceptions by treating causality as an idea which is attributed to the antecedent and contiguous occurrence of external events. He concluded that "a cause is an object precedent and contiguous to another, and so united with it that the idea of the one determines the mind to form the idea of the other..." (Hume, 1740: 116). This formulation has been referred to as the "constant conjunction formulation" (Lana, 1969:8).

The conflict between the idealist and the materialist concepts persists. While many social scientists utilize an idealist view of causality, others insist that science must involve the assumption of an external reality that is independent of an individual's perception of it (Nettler, 1970:93; Popper, 1972:38; Bunge, 1973:23). Part of this insistence derives from the thought that idealism seriously abrogates the concept of consequences (Nettler, 1972b:3-7) and vitiates the idea of social intervention.

There have been various attempts to alleviate the conflict between idealism and materialism. Galileo inverts the question of causality to show that causes can be known by their removal (1623:265), that is, that disappearance of the effect confirms causality. This procedure involves both an epistemological rule and an ontological assumption. One can agree with this approach and assume that causality is an ontological phenomenon that can be attributed to events with varying degrees of accuracy depending upon the epistemological criteria used.

Productiveness

One of the essential qualities of causality is productiveness. Since Plato defines cause as that which "creates," productiveness has become a core element of causality (Bunge, 1959:46). Aristotle refines the notion and calls it "efficient cause," that is, an agent which provides the primary source of change (1930:194). This early attempt to formulate the notion of causality in terms of an external compulsion or an actively producing force eventually leads to the common proposition that productiveness is the "kernel of causation" (Bunge, 1959:46). It also leads to the view that causes and effects are bound together by an ontological force which provides an intuitive sense of "mustness" (Madden, 1960:202). In simpler terms, causes and effects are considered to be connected rather than merely conjoined (Reichenbach, 1959:111). This notion of productiveness is fundamental in science as well as in common belief (Bunge, 1959:46; Nagel, 1961:75).

Association

In causality, association refers to the belief that causes and effects occur together, and that they are conditional, invariable, and non-spurious. That is, causes are said to be associated conditionally in the sense that if the causes prevail, then effects occur (Russell, 1914:219; Braithwaite, 1953:314; Bunge, 1959:36). This association is purported to be the lawfulness of causes and effects (Lana, 1969:30). There is a second sense in which causality is held to be lawful: if causes and effects occur for single events, they will also occur for the class of events of which the single one is a member. This is called the generalizing lawfulness of cause and effect (Nettler, 1970:147).

The second element of association, invariability, is satisfied provided that if the cause occurs, then the effect always occurs. Invariable associations make it possible to separate causal laws from other scientific laws (Nagel, 1961:76); they are distinct from probable associations, which may be deterministic but not causal. Invariability does not mean "forever," as a projection into the future, but rather "without exception" or "in all cases" (Bunge, 1959:38). Further, invariability does not imply that "everything has a cause" (Bunge, 1959:4).

In addition to being conditional and invariable, the association between cause and effect must also be non-spurious.

It must be genuine, and not the consequence of other factors (Simon, 1957:37; Rosenberg, 1968:24; Nettler, 1970:154). Contemporary students of causality have paid much attention to this particular aspect. It is also a very important element in the deterrence hypotheses and, therefore, receives special attention in a subsequent section.

Sequence

The role of time in causality has been the subject of continued controversy, although it is commonly accepted that causes precede effects and that a "finite time-interval" must be involved (Russell, 1912:175; Samuelson, 1965:102). Time sequence has also been considered of little importance (Alker, 1968a: 211). Perhaps some of the controversy arises because descriptions of change usually involve time sequence, but causality need not necessarily be involved (Ducasse, 1960:223). On the other hand, time is part of the definition of causality (English and English, 1958:77). Time is also held to be an element of its common-sense observation (Nagel, 1961:74). In dealing with real phenomena as opposed to symbols, it is legitimate to distinguish between the cause as antecedent and the effect as consequent since time is irreversible (Timasheff, 1959:149). Some researchers suggest that causes and effects may occur simultaneously (Bunge, 1959:63; Zetterberg, 1965:70).

The idea of time sequence is common to both materialistic and idealistic orientations. The constant conjunctive version recognizes the succession of events in time (Hume, 1740:116).

Bunge claims that the concept of causality adopted by scientists within the last century is one which entails the idea of a time lag between cause and effect (1959:63). On the other hand, Scriven places the meaning of causality in the logical structure of statements and on their truth or falsity. He argues that causality does not involve time sequence (1968: 2). Similarly, Simon maintains that models of causality comprised of equations not representing the "real world" have orderings which may be devoid of time sequences (1957:12). However, no student of causality has proposed any criterion for the determination of time sequence, which remains arbitrarily determined (Nagel, 1965:20). That is, there are no criteria by which we can cut time. Hence, we do not know how to stop a regression in the search for causes nor do we know how close in time an event must be to its alleged effect before it can legitimately be called a cause.

Contiguity

Since causes and effects are believed to occur in spatial proximity, causes and effects separated in space are held to be connected by chains (Bunge, 1959:63; Nagel, 1961:74). Although causes occur in space, they are not synonymous with it. As with time sequence, however, criteria for the determination of contiguity have not been set out.

Asymmetry

Asymmetry is the criterion concerned with the direction in which causality operates. Causes are sometimes defined as producing effects in a unidirectional and irreversible flow (Bunge, 1959:39; Nagel, 1961:74; Simon, 1968:353; Alker, 1968a: 211). Simon expostulates that direction or asymmetry should not be confused with time sequence (1968:353). Even though cause is often described in terms of time sequence, and even though the time sequence may provide evidence for asymmetry, the direction of influence is itself independent of time. Simon supports this distinction by claiming that common sense and actual usage of the idea of cause do not involve any consideration of time (1957:12). However, as pointed out in the discussion of time sequence, Nagel considers time to be part of the common-sense view of causality (1961:74).

Not all students consider causality to be asymmetrical. Lerner, for example, argues that causes and effects can be symmetrical, that is reciprocal (1965:7). Other dissenting views refer to reciprocal causes as circular causes (Timasheff, 1959:148), non-recursive causes (Alker, 1968a:217), or as alternating asymmetry (Susser, 1973:68).

Uniqueness

Causes have been considered as unique: one cause produces one effect. This criterion was originally presented by Aristotle (1930:194) and affirmed by Hume, who maintained that the "same cause always produces the same effect, and the same effect never arises but from the same cause" (1740:116). Uniqueness, therefore, is the criterion that demands a one-to-one correspondence of cause and effect (Bunge, 1959:41). This criterion seems to be in keeping with the popular view of causality, that a single event always leads to another single event (Selltiz et al., 1960:80; Nagel, 1965:19).

The one-to-one relationship of cause and effect has been questioned by students who claim that an effect may have (a) plural causes (several alternative causes any one of which can produce the effect); (b) co-causes (two causes which are required to produce the effect); or (c) multiple causes (three or more causes which are required to produce the effect) (Bunge, 1959:122; Scriven, 1966:259). Bunge points out that multiple causes can result in a gradation ranked as "the primary cause, the first-order perturbation, the second-order perturbation, and so on (1959:121).

The case for co-causes, and plural and multiple causes has been opposed by proponents of uniqueness, who argue that these interpretations are the result of vagueness and imprecision, or sloppy analysis and untrained eyes (Russell, 1912: 180; Cohen and Nagel, 1934:27). Bunge argues that the notion of multiple causes results in the loss of causality's meaning after only a few causes or chain "branchings" (1959:125). Opponents of multiple causality also seem to be concerned with the possibility that it invites statistical determination but obscures causal relationships per se (Bunge, 1959:125).

Necessity

Necessity, or that which must be present before the effect can occur, was introduced by Galileo, who points out that the necessary nature of the cause can be seen when, if the cause is removed, the effect disappears (1623:265). Necessity is believed to be both a common requirement for causality and its most rigorous criterion (Cohen, cited by Dray, 1964:74; Nettler, 1970:146). The importance of necessity can be seen in the many other criteria which entail it. Necessity has been defined in terms of productiveness (Bunge, 1959:40) and lawfulness, and as the "mustness" essential for causality (Nagel, 1965:20; Selltiz _et al_., 1960:81; Nettler, 1970:147), as well as an inextricable part of uniqueness (Bunge, 1959:39).

Calling a cause necessary if it is productive, lawful, or unique does not add anything new to the criteria of causality. These considerations, however, all entail the essence of necessity, that is, its _imperative_ nature.

Sufficiency

Sufficient causes are those which are "enough" to account for the effects. This criterion has at least two meanings. The first meaning, on which most discussions focus, concerns the number of causes required to produce the effects. Like necessary causes, sufficient causes have been held to be unique, plural, co-causes as well as multiple causes, all of which have been held to be invariable and contingent. Some students consider causes to be sufficient when they are unique (Hirschi and Selvin, 1967:121; Selltiz _et al_., 1960:81). Zetterberg views sufficiency in terms of alternative causes, any one of which is capable of producing the effect (1965:71). Hobbs defines sufficient causes as co-causes (1655:94). Scriven maintains that sufficiency requires three or more causes to produce the effect, resulting in multiple causes (1966:259). For others, sufficient causes are invariable and contingent upon other factors (Nettler, 1970:146; Scriven, 1966:249). On the other hand, Susser holds sufficiency to mean that no additional factors are required to bring about the effect (1973:46).

There is nothing new in describing sufficient causes in terms of co-causes, plural causes, multiple causes, or any of these attributes since they are entailed by other criteria, each with its own requirements.

Another conception of sufficiency refers to the amount of a causal variable (or a minimum combination) that is required to produce the effect. If the amount is quantifiable, then sufficiency becomes a measure of causal power. This conception will be treated in detail in a subsequent section.

Contingency

The contingency criterion holds that causes produce effects only when in conjunction with some other factors (Bunge, 1959:48; Zetterberg, 1965:71). Nagel calls these other factors "contingently necessary conditions" (1961:559), but Lerner, commenting on Nagel's definition, claims these factors refer exactly to necessary conditions (1965:30). Using a similar line of argument, Scriven proposes to call these additional factors contingently sufficient conditions (1964:408).

The contingency criterion is subject to varying support. Bunge challenges the concept by maintaining that, if causes act only in conjunction with other conditions, then the notion departs from the concept of causality, because causes which depend on other conditions are no longer unique (1959:49). However, in some cases the conditions represent only a refined statement of the cause rather than being a separate entity, since they may be dimensions of a single, complex cause.

CAUSAL CRITERIA EMPLOYED BY SOCIAL SCIENTISTS

Although there is no catalogue of the criteria employed by social scientists, we can discuss some which have been assumed or defended in research. Only a few of the criteria outlined above have been used by sociologists, and most researchers of deterrence pay little attention to them.

Externality is usually presupposed in social science research (Nettler, 1970:93; Popper, 1972:38; Bunge, 1973:23). Obviously, without this assumption the idea of consequences is abrogated, and the notion of intervention through social policies is void. Necessity seems to be a criterion to which social science rarely aspires (Nettler, 1970:154); Blalock declares it an impossible criterion (1960:154). However, externality and necessity must be assumed in order to test for causality in deterrence research. Since deterrence hypotheses claim that legal responses are imperative to reduce crime, necessity is involved by definition. Externality is also presupposed, since general deterrence involves the belief that legal responses and crimes exist independently of any particular individual's awareness of them. Hence ignorance of the law is not an excusing condition.

The criteria of uniqueness and asymmetry are rarely explicitly used in social science, yet sociologists tend to assume that causes are few and independent (Nettler, 1974b: #1:p.2). More probably, social phenomena are comprised of many causes intertwined (Nettler, 1974a:250).

It is difficult to defend the criteria of uniqueness or asymmetry in the study of deterrence. One could maintain that legal responses are hypothesized as the sole cause of deterrence. Yet in that case, do certainty, severity, and celerity constitute dimensions of one cause, or do they constitute different causes in themselves? The problems implicit in measuring legal responses suggest that these elements represent different causes. At least they have been treated as such in the literature. However, this recognition leaves unsettled the nature of their causality, whether plural or multiple.

While the deterrence hypothesis is asymmetrical, there is reason to suspect that changing crime rates also produce changes in legal responses. Thus the abolition of execution as a legal response may be the consequence of a decline in the rates of murder (Enker, 1967; Logan, 1972:69).

Sociological causes, be they single causes, co-causes, plural causes, or multiple causes, are likely to be treated as contingent (Scriven, 1964:408; 1966:259), as is the case for deterrence. For example, knowledge of the law forms an imperative contingency in Beccaria's argument (Manzoni, 1964: 29) and is often cited in contemporary studies (Henshel and Carey, 1972). Ball (1972:472) outlines a compendium of possible contingent factors:

> It is the major thesis of this paper that the deterrent effect of a particular type of punishment is an empirical variable dependent upon several factors. The schema which is presented includes the following factors: (1) the social structure and the value system under consideration, (2) the particular population in question, (3) the type of law being upheld, (4) the form and magnitude of the prescribed penalty, (5) the certainty of apprehension and punishment, and (6) the individual's knowledge of the law as well as the prescribed punishment, and his definition of the situation relative to these factors.

The contiguity requirement has been left implicit in most social science studies. It is necessarily assumed that causes operate in space, yet the isolation of these spaces remains problematic. Bunge states that while contiguity is a methodological requirement, it is a theoretical fiction and an ontologically defective concept (1959:129). Contiguity, however, is implicit in all studies of deterrence. Legal responses are usually examined for those places where they are in effect. This obvious element of jurisdiction has seldom been pointed out (Connelly, Goldstein, and Schwartz, 1962: 898).

Further, the isolation of an environment in which causes are assumed to be operative is not the same as the establishment of causes. As Van den Haag points out, there is often a confounding of causes with the location in which they occur (1968:283). He considers the high rates of crime in slums locations, but not necessarily the causes, of crime. The high crime rate in slums may mean no more than the high death rate in hospitals. He argues that hospitals and slums "attract people selectively" but that neither disease in hospitals nor poverty in slums represents the "cause" of the condition. Deterrence researchers have paid little attention to the criterion of contiguity. The consequences of this failure are discussed in a subsequent chapter.

The most common criterion used by social scientists appears to be that of association (Lerner, 1965:7; Nagel, 1965: 18; Lana, 1969:6; Simon, 1957:37). The second most common attempt to establish causality has employed the dual criteria of association and non-spuriousness (Simon, 1957:37; Blalock, 1964; Costner and Leik, 1964; Bourdon, 1965; Duncan, 1966; Land, 1969: Heise, 1969, 1970; Goldberger, 1970). Another set of criteria (considered to be the "best" approach to establish causality) consists of association, non-spuriousness, and sequence (Hyman, 1955; Selltiz et al., 1960; Hirschi and Selvin, 1967:38; Alhardt, 1969:42). Singly, or in combinations, these three criteria are believed to be the most ubiquitous ones in social science (Meehl, 1970:143). There are occasional deviations from these criteria, for example, Stinchcombe (1968:33) substitutes asymmetry for sequence, on the grounds that sequence can show causal direction. Despite such exceptions sociologists and psychologists normally employ non-spurious, sequential correlation as a test of causal relation.

There is little doubt that statements about deterrence involve a conditional and invariable association between legal responses and crime rates. Lawfulness and generalizing lawfulness may be involved depending upon which deterrence perspective is accepted. *Conditional* criteria are reflected in such statements as: if legal responses are certain, swift, and severe, then deterrence will take place. *Invariability* is contained in the belief that, if legal responses are certain, swift, and severe, then deterrence will *always* occur. The two elements of *association*--*conditionality* and *invariability*--are also tied to the *contingency* criterion of causality, that is, the criterion which specifies the conditions under which legal responses produce deterrent effects.

The time *sequence* criterion is obviously important in the study of deterrence. Deterrence perspectives imply that legal responses to crime precede the deterrent effects. This issue has been discussed only sparingly in the literature (Cousineau, 1973:155) and then it is considered, no rationale is offered for the chosen time lag between the responses and the deterrent effects (Savitz, 1958:338; Schwartz, 1968:509; Chiricos and Waldo, 1970). In addition, no consideration is given to the potential differences in time lag for various offenses. No one appears to have discussed the empirical problems of the isomorphism between legal responses, deterrent effects, and time lags.

The three criteria of *association*, *non-spuriousness*, and *sequence* are frequently used as if they were the best minimal criteria for the attribution of causality to the general deterrence hypotheses. However, this set is inadequate: it (a) *attends only to the causes, not to the criteria, for determining effects*; (b) *does not provide freedom from a priori* assumptions about causality, and (c) does not contain the notion of causal power.

The Emphasis on Causes

All discussions of causality seem to focus on the "cause" side of the cause-and-effect issue. Little consideration is paid to the criteria for effects. For example, discussions about single causes, co-causes, multiple causes, or plural causes emphasize associations about the style of causation, while the event which constitutes the effect is rarely considered. Yet it is possible to talk of single, compound, multiple, and plural effects (and their combinations). It has been pointed out that some single causes are more important than others because they have compound effects (Blalock, 1969: 41), but for the most part there are no guidelines to establish criteria or frameworks for effects. The significance of this issue for deterrence rests on the three-dimensional nature of the supposed effect, that is, the crime rates, which can be measured in terms of the number of offenders, the number of offenses, and the summed, weighted gravity of offenses being committed in a jurisdiction during a particular period.

A Priori Conceptions of Causality

Since we do not assume that "all is caused by all," and since we do not believe that infinite regression is a useful idea, we do not search everywhere for causes (Nettler, 1974b,#17:4). Instead, we tend to think of classes of variables as being causal; thus, our thinking of causes is based on *a priori* conceptions (Scriven, 1968:19; Meehl, 1970:381).

A priori causal assumptions often depend on the discipline involved. The biologist looks for causes different from those investigated by the psychologist, the historian, or the sociologist, and within each discipline, various schools maintain and defend particular causal conceptions. For example, the empathetic explicator may find his "causes" of behaviour in the actor's pursuit of goals (Nettler, 1970:37), while the demographer discovers causes in the arrangements of populations (Stinchcombe, 1968:61). Furthermore, within any one school, the attribution of causes may be legislated by theory, knowledge, or intuition (Blalock, 1961:337; Polk, 1962:541). Theories may specify the causal variable and propose the level and scope of its effects (Blalock, 1971:3; Przeworski and Teune, 1970:25). They may refer to particular social systems, such as the family or religion, which, it is held, can only be causally analyzed when they are stable, closed, and conservative (Hillinger, 1968:144), or when they are dynamic (Bunge, 1959:131).

Theories and decisions about what is causal are often prescriptive. Meehl points out that these decisions are often preconceived and that there has "arisen a widespread misconception that we can somehow, in advance, sort nuisance variables into a class which occurs only on the input side . . ." of our causal formula (1970:381). He contends that this automatic assumption is not justified. He argues that sometimes "nuisance" variables could be "consequences" and that removing their influence from a relationship could result in the falsification of a true causal theory (Meehl, 1971:143). For example, he points out that, if a cause were genetic, then controlling for sociological variables that were its consequences would result in a reduction of a valid relationship (Meehl, 1970:382). These a priori conceptions of causality may be the major problem of non-experimental research. Failure to examine these assumptions is so common that it has been called the "sociologist's fallacy" (Jensen, 1969:221).

The Missing Criterion: Causal Power

The attribution of causality is a complex task, and those social scientists who have attended to it have focused on the criteria which identify causal variables. This approach is an incomplete route to knowledge of causes, however, because it does not include knowledge of causal power. Causal power refers to the amount of a cause that is required to produce an amount of an effect. Without knowledge of causal power, the legal, moral, common-sense, and scientific meanings of causality are vitiated. Certainly, causal criteria which make no provision for the force of causality abrogate the etymological meaning of causation and reduce the utility of causal analysis in the evaluation of social policy.

It has been suggested that the joint criteria of association, sequence, non-spuriousness, and sufficiency, entail causal power, where sufficiency refers to the variance in the effect that is "accounted for by the assumed causal complex"

(Nettler, 1970:158). The criteria of association, non-spuriousness, and sequence jointly identify causal variables. However causal power is only entailed when sufficiency refers to the amount of causal variable(s) required to produce a known amount of the effect(s). Causal power requires measurement, while causal variables provide only identification.

This researcher's perusal of deterrence literature reveals not one attempt to meet all four criteria jointly. In fact, there is no attempt to satisfy any three criteria. Needless to say, association has been the most common criterion sought, with some attempts to ascertain association and non-spuriousness.

The next two chapters will focus on the adequacy of social science methodology to satisfy the minimal criteria of association and non-spuriousness. An evaluation of the employment of these limited criteria is justified since they are primary. If their identification is not satisfied, consideration of additional criteria is pointless. However, Chapter Ten will include a discussion of social scientists' attempts to measure causal power by the amount of variance explained by the purported causal variables.

FOOTNOTES CHAPTER SEVEN

[1] *Chamber's Etymological English Dictionary* defines a cause as a noun meaning "that which produces an effect: ground motive, justification: a legal action..." and as a verb meaning"to produce, to bring about, to induce" (Macdonald, 1966:95).

[2] Similar ideas have been expressed by other authors. MacIver explains this by noting

> . . .writers on the subject are generally agreed that legal responsibility requires authorship. . . only in so far as a person is the cause of an act can he be, or at least should be, held legally responsible for it. (1942:242)

[3] *The Random House Dictionary* defines cause as "...a person or thing that acts, happens, or exists in such a way that some specific thing happens as a result; the producer of an effect" (Urdang and Flexner, 1968:214). Hart and Honore note that one feature of common-sense causal explanations is the criteria that causes and conditions are "equally necessary" if the effect is to occur, and that the cause is "invariably" followed by the effect (1959:31).

[4] Nagel elaborates:

> The moral to which all this points is that it is a mistake to rule out as illegitimate the use of case in some indicated sense in one domain, on the alleged ground that the term is not used in this sense in another domain, just as it is a mistake to make the sense of the term as determined by its use in one domain paradigmatic for its use in all other domains. At any rate, it seems to me futile to discuss whether certain things can, while other things cannot, be "properly" said to be causes. (1965:18)

[5] For example, Lenzen notes:

> Causality is a relation within the realm of conceptual objects. The relation of cause and effect refers to conceptual events regardless of the relation of the latter to reality. (1954:6)

Simon's commitment to idealism leads him to consider the problem of causation to be "...logical rather than ontological ..." (1957:50). As a consequence, he deals primarily with the logic of causal models and shows little concern with the extent to which those models fit the "real world" (Nettler, 1970:146). Similarly, Blalock notes:

> One *thinks* in terms of a theoretical language that contains notions such as causes, forces, systems, and properties. But one's *tests* are made in terms of coveriations, operations, and pointer readings . . .one admits that causal thinking belongs completely on the theoretical level and that causal laws can never ben demonstrated empirically. (1961:5)

[6] For example, in Rosenberg's usage, it is appropriate to call something ". . .a cause when an external influence produces a change in some unit" (1968:10). He goes on to note that "the term 'produces' is critical in this definition, for it implies that an efficient, external agent exercises an isolable power on that unit." Lana prefers to think of cause as a kind of connection and makes reference ". . .to a thing being produced by another thing" (1969:9). Similarly, Bunge states: "If C happens, then (and only then) E is also produced by it" (1959:47). The notion of cause as a productive force is also found in Alker's concept of determinativeness (1968a:213).

CHAPTER EIGHT

RESEARCH DESIGN AND SPURIOUSNESS

Social scientists have attempted to satisfy causal criteria through an amalgamation of research designs and statistical techniques. Chapter Seven discussed what must be known in order to attribute causality. Chapters Eight, Nine, and Ten will deal with the problematic nature of adequate fulfillment of some of these criteria.

Frequently, research designs and statistical techniques are discussed together. This chapter and following ones will treat the two elements separately so that each criterion can be discussed in depth. The criteria on which present attention is focussed are non-spuriousness and association. Unless these basic criteria are met, consideration of the other criteria is redundant.

NON-SPURIOUS ASSOCIATION

At this point it is important to recall that spuriousness is a sub-criterion of association. The word spuriousness has several meanings however, and some of these will be examined below.

Reliable co-variation--a fundamental goal of social science --is considered to be the most realistic goal attainable. However, the path leading from the classification or correlation of man's actions to the determination of causes for his actions remains uncharted (Nettler, 1971:209). Whether causal analysis or association is pursued, the critical problem of spurious correlation persists.

Generally, spuriousness refers to the inauthentic, the counterfeit, or the false (Barnhart and Stein, 1964:1171). This meaning obscures as much as it clarifies and several applications of the term are discernable in social science. Some of these meanings concern ways of interpreting correlations. The term spuriousness, in this sense, means that a correlation is interpreted as meaningless or false. Four meanings of the term spuriousness are, (1) attempts to analyze correlations for spuriousness by the manipulation of variables. This chapter will examine the adequacy of research designs to satisfy this criterion; (2) arguments that interpretations are spurious because they are based upon correlations which are the result of faulty research procedures. Chapter Nine will attend to this problem; (3) attempts to analyze correlations for spuriousness by manipulation of data by statistical techniques; and (4) the attribution of spuriousness, wherein critics repudiate the correlations reported in a study because some variables were not taken into account. The latter two approaches to spuriousness are discussed in Chapter Ten. Our task, at this point, is not to judge which of these usages is correct. Instead, we will show that these usages occur in social science and can be related to the study of general deterrence.

RESEARCH DESIGNS

Research designs are ways in which investigators proceed to answer questions primarily by controlling variance. Designs are sets of rules which inform researchers what observations to make, how to make these observations, and how to analyze the resulting data (Kerlinger, 1964:275). The control of variance is important and good research designs (1) produce a wide range of variation in the variables under investigation; (2) minimize or eliminate the variance due to chance factors and measurement error; and, (3) control the variance which may affect the investigator's findings and which results from unwanted variables, extraneous variables, and variables that do not interest the investigator (Rosenberg, 1968:24).

Controlling Variation by Elaboration

The practice of introducing variables into research designs in order to examine their impact on an original relationship between an independent and dependent variable is called elaboration or specification (Rosenberg, 1968:24; Lazarsfeld, 1968). The purpose of elaboration is to decide whether or not the original relationship is "due to" an association with the new variable, commonly called a "test factor" or "test variable." If the investigator decides that the original relationship is due to the third test variable, then the original relationship is called _spurious_.

A relationship may be interpreted as spurious when it is the result of any one of several kinds of test factors. It could be due to an _extraneous variable_, where the relationship between the independent and dependent variables is the consequence of a common variable; or it could be due to a shared component variable, that is, some dependent and independent variables are comprised of sub-variables, such as social class being made up of measures of education, income, and occupation. The original association may be due to the dependent and the independent variable both possessing such a shared sub-variable. In addition, the relationship may be due to an intervening variable, where the independent variable produces the variable which subsequently results in the dependent variable. Furthermore, the relationship may be determined by an antecedent variable--a test factor preceding the independent variable which, in turn, is followed by the dependent variable. An additional possibility is that the relationship between two variables may be hidden because of a suppressor variable. These are test factors which intercede to reduce the real associations between two variables. Finally, test factors may include distorter variables, or variables that, when introduced, convert a positive association into a negative one.

All of these "test factors" are possible sources of false interpretations of associations. The testing of a relationship for the possibility that it results from a third factor is the most common approach to the identification of spuriousness in sociology (Simon, 1957:38; Rosenberg, 1968:23-104).

Spuriousness and the Control of Variables

The control of variables for purposes of testing relationships for spuriousness is a major technical function of research design. Not all designs are comparable in their capacity to control variables and, therefore, to determine spuriousness.

The relative advantages of some research designs will be briefly considered with reference to testing for deterrence.

The Classical Experimental Design

The most powerful methodology for the demonstration of non-spurious relationships is the experimental design. Despite its familiarity, the advantages of the experiment compared with other research strategies are worth summarizing.

First, in terms of the manipulation of variables, the experimenter has control over changes in the independent variable, while subsequent variations in the dependent variables are observed. The manipulation of variables has the advantage of enabling the researcher to proceed directly from cause-to-effect, rather than to move retrospectively from effect-to-cause. In terms of the attribution of causality, cause-to-effect inferences have several advantages over effect-to-cause inferences. Researchers avoid the errors of ex post facto reasoning, and in addition, the temporal sequence of the relationship between the variables is clear, as is their symmetry or asymmetry.

Second, in terms of testing for spuriousness, the researcher can (1) match experimental and control groups by randomization, (2) match subjects, (3) build the control of extraneous variables into the design, and (4) eliminate possible extraneous variables. Along with the ceteris paribus assumption, these control techniques grant non-spuriousness credibility, especially if the experiment can be replicated and yields similar results (Kerlinger, 1964:280).

In summary, the "best" way of identifying a non-spurious association and attributing causality is through the utilization of the experimental design. This premise is reiterated by a variety of social scientists,[1] who agree with Allardt's statement that

> . . .one can start an argument as to whether a causal relationship can ever be demonstrated empirically, but the fact remains that the logic of causal inference is heavily tied to the classical experimental procedure. (1969:42)

In spite of the superiority of the experimental approach for attributing causality and permitting the interpretation of associations as non-spurious, hypotheses about general deterrence, at least as they pertain to serious crimes, are not amenable to experimental study. Unless one imagines a society in which the experimental use of human beings is not bound by ethical constraints or the need for voluntary co-operation, the experimental study of general deterrence must be confined to the study of pecadilloes rather than the serious crimes in which we are most interested. General deterrence hypotheses primarily concern events of significance: murder, rape, assault, and the more serious larcenies. The use of experimental designs to study such events seems unlikely.[2] Such experimental studies as have been conducted have tested for specific, rather than general, deterrence.

Ex Post Facto Research Design

Ex post facto research consists of observations of the dependent variable after the supposed independent variables has occurred. The researchers then attempt to examine the independent variable retrospectively in an effort to determine the possible relationship to, and effects on, the dependent variable (Kerlinger, 1964:360).

Ex post facto research is dangerous because of the possibility of the investigator committing the fallacy, *post hoc, ergo propter hoc*, "after this, therefore because of this."[3] This reasoning jeopardizes interpretations because variables have not been manipulated and therefore the time sequence and the symmetry or asymmetry of the variables are often difficult to determine. *Ex post facto*, or non-experimental designs, are of at least three kinds: cross-sectional (comparative) studies, and static and dynamic time series designs.

Cross-Sectional Designs

The common non-experimental research design in the social sciences is *cross-sectional*, that is, observations are made for one period in time, comparing individuals or groups that have experienced the alleged cause in differing degree.[4] Investigators attempt to determine relationships and test for spuriousness by analysis of the data using statistical techniques (discussed in Chapter Ten) and by making comparisons (Hirschi and Selvin, 1967:41).[5]

Comparative Studies

Several studies of deterrence are non-experimental, cross-sectional, and comparative. Essentially they compare states with a legal provision for capital punishment with states that do not have such a provision (Sutherland, 1925; Sellin, 1950; Schusser, 1952; Barber and Wilson, 1968). Such studies have been summarized by Sutherland and Cressey (1970) and by Bailey (1973). They have been declared invalid, however, by assertions that the states with capital punishment are not homogeneous with those that do not have capital punishment. Differences in crime rate may therefore be due to a variety of factors other than the presence or absence of capital punishment. Such variables as the age-sex composition, degree of urbanization, unemployment and educational level, region, social and cultural backgrounds, are considered to be alternative explanations for differences in crime rates (Bailey, 1973:2). In sum, these studies are believed to have generated spurious interpretations of comparative data.

An early attempt to control for other possible sources of spuriousness between homicide rates and capital punishment was made by Sellin (1967). He argues that the general deterrent effect of capital punishment can be ascertained by examining homicide rates, while "controlling" for all other variables by comparing adjacent states--at least one with, and the others without capital punishment. For Sellin, proximity of states indicates that they are similar. This similarity is assumed to be the consequence of proximity, but it is not independently measured.

There have been several challenges to Sellin's attempt to control for spuriousness (1967). One critic argues that proximity is a poor basis for assuming similarity (Van den Haag, 1968). Further, the presence or absence of capital punishment in adjoining states has itself been cited as evidence that states differ in other respects (Zimring and Hawkins, 1973: 255).

There have been several attempts to test deterrence hypotheses using the "multiple unit comparison" method of controlling for confounding variables (Zimring and Hawkins, 1973: 259). These studies abandon attempts to control for homogeneity of areas and assume that the *ceteris paribus* condition is met by using large numbers and many areas. Zimring and Hawkins outline two modes of "multiple unit comparison:" (1) the comparison of many areas where some are characterized by one kind of legal response and the others by a different kind of legal response, and (2) the arranging of many areas along a continuum of a particular legal response (1973:259). Several students of deterrence have used this latter method of analysis (Gibbs, 1968a; Tittle, 1969; Erickson and Gibbs, 1973).

If it can be argued that these studies meet the *ceteris paribus* assumption, they do not permit conclusions about nonspurious relations. The dependent, independent, and test variables remain measured for only one point in time. As a consequence, the causal order and asymmetry are indeterminable. In addition, the possibility of a relationship being interpreted as spurious because of interaction or reciprocal effects is plausible. *Interaction effects* are said to occur when a particular combination of two variables may depend on the value of one of the variables (Ferguson, 1959:274; Blalock, 1960:256). Rosenberg points out that another possibility is that of reciprocal effects, which are frequently encountered in social science research. Reciprocal relationships occur when

> . . .it is not immediately possible to specify which is the independent (causal) and which is the dependent (effect) variable, but in which causal forces are nonetheless in operation. This is the case in which the two variables are reciprocal, interacting, and mutually enforcing. (Rosenberg, 1968:8)

If legal responses and crime rates have reciprocal effects, and if the dimensions of legal responses--certainty, swiftness, and severity--interact, then the relationships among these variables may be interpreted as spurious if measured at the same period of time.

Meehl points out that the methodology of making comparisons is itself problematic (1970:377). He states that when comparison involves units of analysis that are different on an independent variable, then this difference is itself crucial. The unknown and unidentified variables accounting for the difference in the independent variable are uncontrolled and unmatched. He suggests that the inability of cross-sectional designs to control variables means that it is fundamentally defective for many, perhaps most, of the theoretically significant purposes to which it has been put (Meehl, 1970:374).

Time Series Analysis

Time series analysis is often used by economists. In general, it consists of arranging a number of observations of one (or several) variable(s) in the sequence of their occurrence at successive points in time. Various time intervals are used, and the data are usually arranged to show either the frequency of occurrences or the magnitude of variables for each time interval (Theodorson and Theodorson, 1969:439).

Static Time Series Analysis A static time series consists of observations of both an independent and a dependent variable at the same point in time, for many time periods (Spiegel, 1961:283). The major advantage of time series analysis is primarily that of monitoring changes in variables over time and thereby gaining some notion of their association. There has been some attempts to study deterrence using static time series analysis (Schuessler, 1952; Sellin, 1967; Barber and Wilson, 1968; Ehrlich, 1972).

Dynamic Time Series Analysis Dynamic time series designs consist of measures of the dependent and independent variables with time itself entering the analysis. Lagged endogeneous designs consist of measures of a dependent variable for several points in time. In this case, the variable is itself held to be a consequence of its previous influence. Lagged exogenous designs consist of measures of the dependent variable for different points in time than those used for the independent variable. Here the dependent variable is held to be a consequence of some past influence of the independent variable (Hannan, 1971:75).

Time Series Quasi-Experiments A common form of the lagged exogenous design is the quasi-experiment or the interrupted time series analysis.[7] In this design the "causal" variable is examined as an event or change occurring at a specific point in time, specified independently of inspection of the data. Hypotheses are tested by comparing the dependent variable before and after the supposed intrusion of the alleged cause (Campbell and Ross, 1968:41). This approach has been developed by Campbell and Stanley (1963); applied by Campbell and Ross (1968) and Lempert (1969), and elaborated by Campbell (1969).

The closer researchers come to manipulating variables, the closer they approximate the conditions of a "real" experiment (Campbell and Stanley, 1969:234). While many areas of interest to the social scientist fall outside his scope of manipulating variables, this does not mean that no research can be done:

> Social research frequently encounters the task of evaluating change produced in nonrandomly selected groups by events which are beyond the researcher's control. The social scientist must verify that there has in fact been a change, and that the indicated event is its cause. . . Because in these situations the investigator has no control over the assignment of individuals or groups to "experimental" and "control" situations, the logic of the classical experiment must be reexamined in a search for optimal interpretative procedures. (Campbell and Ross, 1968: 34)

The natural quasi-experiment focuses on the examination of changes in natural settings (Popper, 1959:45; Nagel, 1961:19; Kaplan, 1964:164; Madge, 1965:291; Campbell, 1969). Although the researcher does not manipulate variables, there are manipulations by others and such situations are believed to provide opportunities by causal analysis (Simon and Rescher, 1966: 330; Susser, 1973:47).

Spuriousness and the Quasi-Experiment One touted advantage of the quasi-experiment is the opportunity to evaluate the interpretations of associations for spuriousness by means of the consideration and elimination of "plausible rival hypotheses." A rival hypothesis is a variable which could explain the effects tentatively attributed to the independent variable. Because of the lack of experimental control in this design, researchers are "obliged" to be aware of, search for, and test, results for possible spuriousness due to plausible rival hypotheses (Campbell and Stanley, 1963:204; Lempert, 1966:112; Campbell and Ross, 1968:53). Campbell and Ross oppose the naive attribution of cause which fails to consider any other explanation:

> A perusal of research. . . would soon convince one that the causal interpretation of data is overdone rather than underdone, that plausible rival hypotheses are often overlooked, and that to establish the temporal antecedence-consequence of a causal relationship, observations extended in time, if not experimental intrusion of X, are essential (Campbell and Ross, 1963:235; emphasis mine).

Legal Impact Studies Some students of deterrence claim that an opportunity for using the quasi-experiment occurs when there are changes in the enforcement of the laws or when legislators alter existing laws. Campbell (1969) makes a case for considering such "reforms as experiments:"

> By and large, when a political unit initiates a reform, it is put into effect across the board, with the total unit being affected. In this setting, the only comparison base is the record of previous years. The usual mode of utilization is a causal version of a very weak quasi-experimental design, the one-group pretest-post-test design. (Campbell, 1969:413)

Similarly Meehl lauds the quasi-experiment for its potential in studying the impact of laws:

> While the sources of error in "common knowledge" about behaviour are considerable, the behavioural sciences are plagued with methodological problems which often render their generalized conclusions equally dubious. Legal applications of generalizations from experimental research on humans and animals in laboratory contexts often involve risky parametric and population extrapolations. Statistical analysis of file data suffers from inherent interpretative ambiguities as to causal inference from correlations. Quasi-experiments in the "real-life" setting may often be the methodologically optimal data source. (1971:65, emphasis mine)

Unfortunately, the law is rarely subject to abrupt changes and consequently, there are few opportunities to use the natural quasi-experiment. Two possibilities, however, come to mind. The first involves the disruption of the usual legal responses, due to an event such as war or a police strike. A second possibility involves change in legal responses as a consequence of legislation. It is my contention that only the last kind of situation has some potential for testing hypotheses about general deterrence, and that even in such a case, we are unable to argue confidently that an interpretation is non-spurious.

Societal Disruption Some circumstances, such as war and political revolts, may result in changes in legal responses to crime through large-scale disruption of societal arrangements. For example, Kinberg provides documentation of lawlessness during wars, and similarly, Trolle describes the disruption of law enforcement for seven months in Denmark during the Second World War when the German Army arrested the entire Copenhagen Police Force (Andenaes, 1966:962). Although increases in criminal behaviour may be documented during these disruptions, such situations do not provide evidence for non-spuriousness. The assumption of "other things being equal" is plausible, but not assured. The changes in crime rates may be due to a host of additional factors that make conclusions about deterrence questionable. In addition, because it is impossible to differentiate between initial offenders and recidivists, it is impossible to know whether the increases in crime rates are due to the failure of individual or of general deterrence.[9]

Police Strikes One example of variation in legal responses to crime occurs when police surveillance is disrupted or eliminated. Although such occurrences are rare, the literature on deterrence makes reference to some examples. One of the earliest involves the police strike in Liverpool in 1919,[10] and more recently, the Montreal police strike (Clark, 1969). Although accounts of these situations may provide some basis for speculation, they are of limited value for inferences about deterrence. The period of disruption is often very short and if an increase in crime is documented, it is still impossible to distinguish the *amount* of increase due to a failure of *general* deterrrence (Zimring, 1971:68).

Legislative Changes One approach to the study of general deterrence involves observations of crime rates over short time periods before and after legislative changes in legal responses. In order for such research to be relevant, it should involve serious offenses. Thus Chambliss (1966) evolves an appropriate research design, but the offenses he considers are parking offenses on a university campus and the responses, increases in fines.[11] In contrast, Schwartz (1968) investigates the effects of legislative changes in the penalties for forcible and attempted rape.[12] His data cover a period of five months, and he concludes that there are no significant changes in the patterns of rape. Unfortunately, the value of Schwartz's study is limited by his assumption that the legislative changes in penalties for rape reflect comparable changes in actual legal responses. Thus, although Schwartz refers to the provisions of the legislation for new sanctions, there are no indications of the frequency with which the courts imposed them (1966:509).[13]

One important quasi-experimental study is the "Connecticut Speed Crackdown" (Campbell and Ross, 1968; Glass, 1968). Legislative changes were introduced with the intention of deterring speeders by "getting tough" with them: increasing fines, permitting prison sentences, and suspending driver's licenses. Here, the discrepancy between the law-in-action and the law-on-the-books becomes apparent. The research shows that the law enforcers are reluctant to act when sanctions are "too severe." As a consequence, there is a reduction in the certainty and celerity of sanctions. While observations of changes in the law-on-the-books may indicate the possibility of comparable changes in the law-in-action, such legislative reforms do not guarantee changes in actual legal responses to crime.

A second approach to legislative changes is the analysis of data for long time periods. Barber and Wilson (1966) use this approach in attempting to correlate capital punishment with rates of murder in Queensland from 1860 to 1931. Barber and Wilson (1966) recognize the problems of "slippage" between the law-in-action and the law-on-the-books, and consequently employ the legal status of the death penalty, rates of conviction, and rates of execution. Unfortunately, even if one is willing to overlook the limitations of official statistics *per se*, one cannot overlook the problems of records over a century old. Trying to derive data from such records and assuming them to be reliable, valid, and comparable from one decade to the next, seems dubious.

It seems clear, that studies of interrupted time series, such as societal disruptions, police strikes, and the several studies of legislative reforms are all vulnerable to charges that, due to the lack of controls in these studies, their reported associations may be interpreted spuriously.

DETERRENCE AND RIVAL HYPOTHESES

Campbell outlines several plausible rival hypotheses (some of which are relevant to studies of deterrence) to be considered in assessing causality from quasi-experiments. They are hypotheses assessing the effects of history, maturation, instability, testing, instrumentation, regression artifacts, and diffusion (1969:411). These problems are common to both interrupted and all time series analyses.

Campbell's "rival hypotheses" are described below, along with their possible relevance for studies of deterrence (1969: 411).

(a) History: Events other than the experimental treatment, occurring between pretest and posttest, may provide alternate explanations of effects. No study of deterrence, for example, considers changes in assault and homicide rates in terms of the development and deployment of medical resources. I am assuming that medical resources may transform "close homicides" to assaults.

(b) Maturation: Processes within the respondents or observed social units may produce changes as a function of the passage of time--changes recognized as growth, fatigue, and secular trends. This possibility for studies of deterrence is related to the changing size of the population-at-risk and has been examined in Chapter Five. In addition, maturation could apply to the problem of delinquency versus adult criminality. For example, when does an age group become subject to the deterrence philosophy, and when do they supposedly become vulnerable to deterrent effects? Further, maturation could also relate to the question of how long deterrence effects are supposed to last--a few days, weeks, years, or for a lifetime?

(c) Instability: Unreliability of measures may result in pseudo effects that are incorrectly interpreted as real. This plausible hypothesis is considered in a subsequent chapter.

(d) Testing: The effect of taking a test upon the scores of a second testing, or the effects of publication of a social indicator upon subsequent readings of that indicator, are possible sources of spuriousness, but do not seem relevant to studies of deterrence.

(e) *Instrumentation*: Changes in measuring instruments or changes in the observers or scores used may produce changes which are erroneously attributed to the supposed cause. These forms of spuriousness overlap with *instability*, and will be discussed in the chapter on correlations and faulty research procedures.

(f) *Regression artifacts*: Changes in measures may occur when persons or treatment units that have been selected on the basis of their extreme scores tend to have scores that are less extreme when observed a second time because of normal fluctuations in measures over time. This rival hypothesis, also discussed in the section on procedural spuriousness, is believed to be the most recurrent kind of spuriousness in quasi-experiments (Campbell, 1969:412; Susser, 1973:75).

(g) *Selection*: The differences among the kinds and size of comparison groups may result in correlations that can be spurious because of real artifacts and the problem of isolating social environments. This form of spuriousness is common in deterrence research because of the inappropriate use of geographic areas as units of analysis. Detailed discussion follows in Chapter Nine.

(h) *Experimental mortality*: This rival hypothesis concerns the differential loss of subjects. In deterrence research this involves changes in the population-at-risk due to mobility and the age-sex composition of the units being compared.

(i) Diffusion: This plausible rival hypothesis is related to studies that use control areas or populations which are in proximity. Diffusion may occur when the factors that are thought to affect only the experimental unit also affect the control unit (Campbell and Ross, 1969:46). For example, in explaining the results of the "Speed Crackdown," diffusion is the potential effect which the crackdown in Connecticut may have on the adjacent control states. The idea of diffusion is similar to the concept of "contagion" in the research of Berkowitz and Macaulay (1971).

Spuriousness and Plausible Rival Hypotheses

The critical disadvantage of the natural quasi-experimental approach is that there are an indefinite number of rival hypotheses. The researcher cannot consider all of them. Rather, he may be using some theoretical framework which entails only plausible hypotheses. Campbell and Stanley point out that "because full experimental control is lacking, it becomes imperative that the researcher be thoroughly aware of the specific variables his particular design fails to control" (1963:204).

Unfortunately, Campbell and Ross do not consider the sources of plausible rival hypotheses, nor do they discuss the questions of what makes hypotheses plausible. The most telling criticism of this prescription is that the selection of rival hypotheses is usually based upon some *a priori* assumption about causes. A rival hypothesis is a possible test variable which should be controlled for. Deciding whether to do this depends in turn upon one's presuppositions. As pointed out in Chapter Seven, these presuppositions may be as problematic as the hypothesis under test (Meehl, 1971:147).

Meehl argues that the automatic assumption that some variables should be controlled in order to determine the true causal relationship is indefensible:

> There is no general justification for the routine assumption that demographic and allied variables.. ....should be taken as always functioning solely on input side and therefore, as always appropriately "controlled" by a matching operation or by some similar type of statistical correction. (1970:383)

The question, then, remains of how to differentiate relevant and irrelevant or plausible and non-plausible hypotheses. This question ultimately must lead back to questions about criminogenesis and crime prevention. With the large number of variables believed to be the causes of crime, the non-experimental testing of hypotheses by comparing them to alternatives may be an infinite process. At some point, unable to devise a "crucial experiment," the process must stop arbitrarily, and at that point, "causes" are "best" thought of as assumptions about possible causes.

TIME AND DETERRENCE

As pointed out in Chapter Seven, students of deterrence have neglected the problem of the length of time incurred between the imposition of legal responses and resultant deterrent effects (Cousineau, 1973:153). There seems to exist, however, an assumption that deterrent effects do not occur simultaneously with the imposition of sanctions, but that they follow after some unknown interval. Unfortunately, we have no guidance as to how long this time period might be before changes in the certainty, severity, or celerity of official responses to crime may be expected: ". . .to seep down through the criminal element who will then think twice. . . .bringing about the desired deterrent effect" (Schwartz, 1968:509).

Our review of research designs reveals that a few researchers have attempted to time-lag their observations, using a variety of lags without justification for the time periods chosen. Thus, Dann suggests that, with reference to capital punishment for murder, deterrence should be most in evidence in the _days_ immediately following the execution (Savitz, 1958: 338). Savitz (1958) also uses time-lagged analysis to examine the rates of homicide following publicity of the sentencing to death of convicted murderers. He computed the homicide rate for _each week_ for _eight weeks_ preceding and following the publicity. Chambliss (1966) looks for deterrent effects of increased finds on parking violations for _three_ time periods of _two weeks_ each. Schwartz's (1968) study of rape, examines deterrent effects in _each six-week_ period for _three periods_. Campbell and Ross (1968) inspect data for changes due to supposed deterrence for _four three-month_ periods for _one year_. Chiricos and Waldo (1970) attempt to assess the deterrent hypotheses on the basis of data from the _Uniform Crime Reports_. They use indexes of the severity and certainty of sanctions for the years 1950, 1960, and 1964, and then look for deterrent effects in terms of the changes in the crime rate for the *three years* following each time period. Barber and Wilson (1968) inspect homicide rates for *each year* for *ten years* following abolition of capital punishment in Australia. None of these researchers discuss the time lag chosen. All decisions appear to be arbitrary.

Time as a unit of analysis is modifiable and, therefore, subject to arbitrary choice of length. If the interval of time does not match an interval of causation, and if the theoretical issue of the time lags between the various legal responses and supposed deterrent effects for various offenses is not solved, then no study of deterrence to date can be instructive. Even if the time lag problems are solved, the question that remains is as to how long deterrent effects are supposed to last.

Finally, where data are in the form of time series--lagged or not--we do not know how to interpret asymtopes, non-linear dependencies, threshhold effects, and second-order interactions in relation to causality.

No research designs to date have been able to assure us that the correlations between legal response and crime can be confidently interpreted as non-spurious. In addition, the question of the degree of isomorphism among legal responses, deterrence effects, varieties of offenses, and time lags remains open. Further, researchers may report associations that can be interpreted as spurious because of faulty research procedures. This problem is the focus of our attention in the next chapter.

FOOTNOTES CHAPTER EIGHT

[1] For example, see Greenwood (1945:19-20); Popper (1959: 45); Hart and Honore (1959:27); Blalock (1964:95-96); Scriven (1966:258); Campbell (1969:409); Elhardt (1969:42); Ryan (1970: 57); and Susser (1973:71).

[2] Most of the experimental studies that are cited in support of general deterrence are laboratory experiments concerning learning theory. Unfortunately, these are of little use in examining the deterrent hypotheses, for in addition to the problem of generalization, the studies concern the impact of sanctions upon the person who offends. Thus, while they may tell us about individuals' deterrence, they are not applicable to general deterrence hypotheses.

[3] Critics of this kind of research also point to the dangers of ex post facto reasoning. Heise notes that these designs focus on supposed cause-effect changes that have already occurred (1969:44). Once an event has occurred, it is obvious that some of the existing conditions brought it about. However, the identification of such variables is problematic.

[4] Coleman argues that cross-sectional studies are not sociological (1964:88). Because these designs aggregate individuals without regard to social contents or environments, they simply result in aggregate psychology, where the means, proportions, and sums are used inappropriately as if they were sociological variables.

[5] Cross-sectional studies rarely permit determination of causal sequence, although there may be analytic solutions to this problem (Hirschi and Selvin, 1967:57; Rosenberg, 1968:11).

[6] In addition, if no changes occur in legal sanctions and there is no "manipulation" of the independent variable, then asymmatry cannot be deduced even if sequence is determined.

[7] This design permits the ordering of purported causes in terms of sequence and lends credibility to asymmetry.

[8] An extensive report on this situation is provided by

Jorgen Trolle, who at that time was head of the Copenhagen state prosecutor's office. His 1945 book Seven Months Without Police is not available in English, but Andenaes translates and summarizes his description:

> The general crime rate rose immediately, but there was a great discrepancy between the various types of crime. In 1939 only ten cases of robbery were reported in Copenhagen, but by 1943 about ten robberies were committed each month, as a result of war-time conditions. But after the German action against the police, the figure rose to over a hundred per month and continued to rise. Larcenies reported to the insurance companies quickly increased tenfold or more. The fact that penalties were greatly increased for criminals who were caught and brought to the courts did not offset the fact that most crimes were going undetected. Crimes like embezzlement and fraud, where the perpetrator is usually known if the crime itself is discovered, do not seem to have increased notably. (Andenaes, 1966:962)

[9] For example, while societal disruptions and police strikes may provide some basis for speculation, they are of limited value for the study of deterrence. If an increase in crime is documented, it is still impossible to distinguish the amount of increase due to a failure of general deterrence (Zimring, 1971:68). Because one cannot differentiate between initial offenders and recidivists, it is impossible to know if the increases in crime rates are due to the failure of individual or of general deterrence. This problem was recognized by Kinberg, who in basing his observations on studies of the French Revolution and other political upheavals, holds that the rate increases primarily because existing criminal and social elements take advantage of the unusual circumstances, but that men who were "potential criminals" before the crisis also make a contribution (Kinberg, 1935, cited by Andenaes, 1966:958).

[10] Starting at midnight on July 31st, 1919, nearly half of the Liverpool police were out of service. An official report on the incident comments:

> . . .the strike was accompanied by threats, violence and intimidation on the part of lawless persons. Many assaults on the constables who remained on duty were committed. Owing to the sudden nature of the strike the authorities were afforded no opportunity to make adequate provision to cope with the position. Looting of shops commenced about 10 p.m. on August 1st, and continued for some days. In all, about

> 400 shops were looted. Military were requisitioned, special constables sworn in, and police brought from other centres. (Mannheim, cited by Andenaes, 1966: 962)

[11] Chiricos and Waldo (1971:201; footnote #1) points out that one of the possible sources of spuriousness in this study is the failure to take into consideration the opening of several new parking lots and the expansion of old lots on the campus.

[12] These changes were precipitated by a series of particularly offensive crimes. Schwartz reports that:

> On April 3rd, 1966. . .three Negro men broke into a West Philadelphia home occupied by a eighty-year-old widow, her forty-four-year-old daughter and fourteen-year-old granddaughter. . .the intruders viciously beat up and raped both women and the child, ransacked and looted the home. . .Each of the three victims were ferociously dragged and thrown about. . .the upstairs and downstairs were spattered with blood. The grandmother later died of her wounds. (1968:509)

The degree of atrocity associated with these crimes led to an exceptionally intense public outcry, voiced and fanned by the coverage given them by The Philadelphia Inquirer. "By the middle of April the Palm Sunday Rape in West Philadelphia had become a cause celebre throughout the state" (Schwartz, 1968: 109). The state legislature devoted several special sessions to considerations of increases in penalties for rapists, and within two weeks the Pennsylvania Penal Code of 1939 had been amended, with dramatic increases in prescribed legal penalties for rape.

[13] For cases without bodily injury, the maximum sentence for rape was increased from 15 to 20 years imprisonment, and the maximum sentence for attempted rape was increased from 5 to 7 years imprisonment. For cases with bodily injury, the maximum sentence for rape was increased from 15 years to life imprisonment, and the maximum sentence for attempted rape was increased from 5 to 15 years imprisonment. For persons convicted more than once for rape or attempted rape, the maximum sentence was increased from 5 years to life imprisonment: when considering inveterate offenders, no distinction appears to be made between cases involving or not involving bodily injury. For a complete discussion of these legislative changes, see Purdon's Pennsylvania Legislative Service (State of Pennsylvania, 1966:27-28).

CHAPTER NINE

CAUSALITY AND RESEARCH PROCEDURES

The attribution of causality is hampered when researchers fail to carry out their projects correctly. *Procedural spuriousness* is the interpretation of an association as probably false because it is the result of improper research procedures. Procedural spuriousness arises from mistakes in computation, the use of inappropriate sampling units, and data containing measurement errors (Rosenberg, 1968:28; Meehl, 1971:146). Hirschi and Selvin call this type of spuriousness "contamination" (1967:77). Meehl argues that this is the only real kind of spuriousness (1971:146). The discovery of procedural errors in any study means that the findings are compromised.

The findings of many studies of deterrence are probably false because of two common forms of procedural problems--measurement error and inappropriate sampling units.

MEASUREMENT ERROR

Most studies of deterrence are impaired by the unexamined problems associated with the poor quality of data used, aggregation artifacts, regression artifacts, and inappropriate indices of crime and legal responses.

Measurement Error and Secondary Data

> The main problems concerning criminal statistics are not matters of detail but relate to quite fundamental concepts. . . (Wilkins, 1965:277)

The data used by most studies of deterrence are unreliable because they are derived from secondary official statistics. Most research on general deterrence concerns crime in the United States and uses two major sources of data: the Uniform Crime Reports, which provide statistics on the number and kind of persons subject to arrest; and the National Prisoner Statistics, which deal with the number and kind of persons incarcerated. The work using such data begins with Gibbs (1968a) and includes the works of Tittle (1969), Gray and Martin (1969), Chiricos and Waldo (1970), Bailey, Gray and Martin (1971), Bean and Cushing (1971), and Logan (1972). A second group of studies from econometric literature also uses these data sources (Silver, 1974), including the works of Sjoquist (1970), Ehrlich (1972), Phillips and Votery (1972), Votery and Phillips (1972), Sjoquist (1973), Ehrlich (1973), Carr-Hill and Stern (1973), and Orsagh (1973). Exceptions are Sellin (1961, 1967a), who uses the American Vital Statistics Reports, and Teevan (1972) and Jayewardene (1972), who employ Canadian secondary data from Correctional Institutional Statistics. Barber and Wilson (1968) use the Criminal Statistics for the State of Queensland.

The limitations of using official statistics as data have been outlined by a number of authors (Sellin, 1951; Cressey, 1957; Beattie, 1960; Kitsuse and Cicourel, 1963; Wolfgang, 1963; Wolfgang and Sellin, 1964; Wilkins, 1965). There are a number of technical problems, such as incomplete data, lack of consistency in reporting techniques, and lack of comparability due to the grouping of heterogeneous offenses and offenders in supposedly homogeneous categories. For example, two discrepancies in the American sources are noted by Tittle (1969:412). The Prisoner Statistics sometimes report assault separately and sometimes combine it with kidnapping, whereas the Crime Reports include both in the category of "aggravated assault." In addition, the Prisoner Statistics group all felonious sex offenses, while the Crime Reports provide data only for forcible rape.

The artificial grouping of heterogeneous offenses into supposed homogeneous categories has two major implications for studies of general deterrence. In the first place, the more inclusive offense categories found in the National Prisoner Statistics result in artificially inflated measures of certainty (Bailey, Gray and Martin, 1971:286). In other words, the Prisoner Statistics have higher numbers for aggravated assault than do the Crime Reports, because the former include a larger number and variety of offenses.

The second, and most important, implication of these groupings of offenses is that they impede conclusions about the effectiveness of the death penalty as a deterrent. In the Uniform Crime Reports, the offense category of "criminal homicide" includes both "murder and non-negligent manslaughter" and "manslaughter by negligence." The first grouping includes "all willful homicides distinguished from death caused by negligence." However, in regard to the deterrent effects of the death penalty, such grouped statistics are inappropriate, since only murder carries the death penalty, whereas the other two forms of criminal homicide, non-negligent and negligent manslaughter, do not (Bedau, 1967:61). In spite of this heterogeneity, many studies of deterrence use "criminal homicide" as if it were a single offense category. Some researchers ignore the distinction altogether (Sellin, 1967; Tittle, 1968; Bailey, Gray, and Martin, 1970). Others acknowledge the problem, and attempt to "resolve" it by assuming a constant ratio of murders to manslaughters over time and place (Gibbs, 1968a; Chiricos and Waldo, 1970; Fattah, 1974).

However, the ratio of the two offenses is not constant and, in fact, varies from 30 to 85 per cent. Such diversity leads Bedau to lament that

> . . .since so much of the debate over the deterrent efficacy of the death penalty seems to turn on the validity of this assumption, it is regrettable that there is so little evidence that can be offered to support it. (1967:72)

Most researchers acknowledge the limitations of the official statistics but justify their use as the "best" available. An additional justification is offered by Bailey, Gray, and Martin (1970), who try to take the limitations into account in their analysis, and appeal for more research, undertaken in idealized conditions. Unfortunately, all of the problems with official statistics cannot be overcome by sophisticated analysis or restrained generalizations. This is especially true when the focus is on testing general deterrence hypotheses. Only recently have researchers in deterrence acknowledged that

> . . .it might well be that the whole question of the relationship between these variables. . .is inadequately assessed through the use of police and prisoner statistics. (Bailey and Smith, 1972: 539)

For the purpose of measuring legal responses, official statistics are of limited utility. Apart from technical flaws, they have three irremediable limitations: first, they focus on one kind of sanction, incarceration; second, they do not differentiate between initial offenders and recidivists; and third, they do not allow adequate measurement of the certainty and celerity of responses. Data at all stages of the administration of justice are required, rather than data on the final stage of incarceration, to gauge these responses.

Official statistics do not permit the association of an individual with an offense, nor the following of an individual over time through the judicial process. They cannot be used to assess certainty at different stages of the process. Thus, the Uniform Crime Reports indicate the number of offenses committed in a given year, and the number of arrests made for such offenses, as well as the number of convictions obtained. However, the arrests in a given year are not necessarily for offenses committed during that year. There is always a certain amount of "slippage" in considering changes in rates.[1]

A necessary condition for the test of deterrent hypotheses is antecedency: if official responses are to have an effect on crime, they must occur before subsequent crimes are committed. The use of official statistics, which base legal responses and crime rates upon the same calendar year, precludes the measure of antecedence.

Measurement Error and Primary Data

The deficiencies of secondary data have resulted in appeals for primary data (Wilkins, 1965:283-284). Thus, Tullock expects data problems to be solved by new crime statistics from the United States Law Enforcement Administration, which are believed to be the basis for a "new generation" of deterrence research (1974:104). Similarly Wellford thinks that once the measurement of crime is refined, the assessment of severity, certainty, and celerity will not be a measurement problem (1974:121). So, too, Zimring and Hawkins appeal for better data from correction and prison systems (1973:335).

While such appeals may be laudatory, primary data will not solve the problem of deterrence hypotheses testing, because as outlined in Chapter Six, even primary data are unreliable.

Measurement Error and Aggregated Data

Most deterrence studies use data which have been aggregated, that is, individual measures are grouped into sums. These aggregates are the amalgamation of reporting units, but how they were tallied is unknown. Unfortunately, variations in measures of association occur because of the ways in which aggregate data have been grouped (Blalock, 1964:78). Small units of data may be grouped into larger units by at least three different methods. Proximity grouping adds together units which are close in space. Random grouping groups units by some random process. Similarity grouping adds together units which are similar on either the dependent or the independent variable. Each method of grouping results in a different magnitude of correlation between the dependent and the independent variable.

For example, the proximity grouping method tends to increase the variation between units relative to the variation within units (Hannan, 1971:45). This mathematical artifact results in a correlation with the independent variable accounting for a larger proportion of the dependent variable than is "real." The change in the amount of variance is sufficient to create a low or zero correlate for small reporting units, which, when grouped, produce a spurious macro-correlation (Hannan, 1971:48). Further, Hannan shows that correlations increase in magnitude up to some limit, as the number of aggregated units increases (1971:36). To date there are no techniques for the control or estimation of this artifact, and deterrent researchers have not attended to these problems.

Measurement Error and Regression Effects

Regression to the mean occurs when a unit of analysis is selected for study because it represents an extreme. Then, any subsequent measurement will tend to be less extreme. When time series data are fluctuating, and a high or low point is chosen for a basis of comparison, then the next variation is likely to be towards the mean, purely as a function of the fluctuations (Campbell, 1969:414). In the analysis of the consequences of social policy, such as in legal impact studies, this artifact is recognized as the most common form of self-deception (Lempert, 1966; Campbell and Ross, 1968:40). Campbell and Ross (1968:40) and Glass (1968:96) found that almost all of the variance in rates in the "Connecticut Speed

Crackdown" was due to regression effect. When deterrence research matches populations or areas, then it is possible that

> . . .as time passes, the values of the outcome variable for both bases and controls will regress toward the mean values for the groups from which each originates. Because matching is presumed to have made other things equal, such divergence is likely to be mistakenly attributed to the unmatched factor being tested. (Susser, 1973:78)

Logan (1971b), in discussing the work of Chiricos and Waldo (1970), argues that these authors erroneously interpret changes in the magnitude of correlations over time as indications of the unreliability of the data. Logan points out that hypothesis confirmation does not require correlations to remain constant over time (1971b:281). However, Logan and Chiricos and Waldo overlook the possibility that the changes in correlation are "regressions to the mean" and "aggregation effects." There is no way of balancing regression effects against aggregation effects to decide how much of the variation in a crime or legal response rate is "real." Finally, no one knows whether regression and aggregation effects interact or whether they are additive.

Measurement Error and Indexical Artifact

Indices which are not independent are a persistent problem in studies of deterrence. If we use the number of arrested offenders as a crime rate, it can be argued that arrest is also a legal response, and thus we have no purely independent measure of either crime or legal response.

The second problem is more complex, but flows from the first. Many of the correlations found in studies of deterrence are artifacts because they are calculated from indices containing a mutual term. This problem has been pointed out by some researchers of deterrence (Tittle, 1969:410-413; Chiricos and Waldo, 1970:210-213; Logan, 1972:67). These authors note that, if the certainty of imprisonment is measured by the ratio of admissions to prison divided by crimes known to the police, and if the crime rate is measured by the ratio of crimes known to the police divided by some population base, then the certainty index and the crime index are not independent. They both contain a common term, that is, crimes known to the police. The correlation between these two ratios is, therefore, artificially inflated because of this common term. Consequently, attempts to interpret such correlations are subject to error.[2]

While this problem has been recognized in the sociological literature, the studies of Sjoquist (1970), Phillips and Votery (1972), Votery and Phillips (1972), and Carr-Hill and Stern (1973) all contain such kinds of unrecognized procedural defects.

SAMPLING UNITS AND SPURIOUSNESS

Many researchers of deterrence produce spurious findings because of inappropriate sampling units. A common sample unit is a geographic area--usually a nation, a state, or a region.[3] These units result in spurious findings because they are subject to areal aggregation artifacts and because they are not sociological units of analysis. Even when social environments, such as the city or neighborhoods, are used,[4] spuriousness may flow from sample contamination and unknown interaction effects.

Areal Artifacts

When geographic areas are used as units of analysis, the correlations derived from such units are artifacts. Not only are the data subject to all previously outlined measurement errors, the correlations obtained from such data are spurious because the magnitude of associations is the consequence of the size or area of the unit. As the area increases, the correlation increases, and it is possible to produce a correlation of any magnitude from zero to unity by selecting areas of appropriate size (Yule and Kendall, 1950:311; Hannan, 1971: 36). Thus, areal sampling gives rise to spurious correlations and for such correlations to make sense, we have to know the ways by which the smallest reporting unit's totals or averages are transformed into the large units, such as the regional, or national, level measures (Hannan, 1971:20). In addition, using aggregates, such as sums, means, medians, and proportions, as if they were measures of psychological or sociological variables, is not justifiable (Coleman, 1964:88).

Isolating Social Environments

Areal units are not strictly sociological variables. Social behaviour is not random; rather, it is the consequence, in part, of socially structured relations. The systematic analysis of these structures separates sociology from psychology. The study of aggregates without regard to social context has been called aggregate psychology rather than sociology (Coleman, 1964:88; Nettler, 1974:138).

Crime and legal response rates are probably best used as sociological variables when they are the result of data for the specific social environments in which they occur. It has been suggested that students of deterrence utilize "natural" sociological environments and the techniques used to measure them (Cousineau, 1973:155).

For example, Hatt defines a natural area as "a special unit limited by natural boundaries enclosing a homogeneous population with a chracteristic moral order" (1946:423). The idea of the existence of homogeneous "natural crime areas" has been employed by criminologists for nearly half a century, ever since Reckless' work on the "natural vice" areas (1926: 164). Bullock examines homicide in terms of spacial and temporal factors and concludes that these variables describe "the natural manner in which the assailant, victim and place become organized into a complex of ecological and interpersonal situations that result in homicide" (1955). Similarly, Moses (1947) compares differentials in crime rates between Negroes and Whites in "socio-economically equated" areas as defined by census tracts. An extensive study of natural crime areas by Schmid (1960a, 1960b) utilizes typologies for the identification of homogeneous social areas. He considers the distribution of 20 different kinds of criminal activities and concludes that "the relation between the six dimensions of the . . . typologies and 20 crime categories conform to similar patterns" (1960b:672). Moreover, if the dimensions are trichotomized as high, medium, and low, then low scores on the three dimensions predict high crime areas (Schmid, 1960b:672-674). Schmid's data also indicate that natural crime areas remain fairly constant over time. Schmid compares two series of offenses for two groups of census tracts for the periods 1939 to 1941 and 1949 to 1951, and concludes that "the basic configurations for the respective series of crime categories show a marked similarity" (1960b:669).

The most recent and most promising attempt to establish a methodology for the identification of homogeneous social areas has been isoplethic mapping. Hoiberg and Cloyd (1971) note that most attempts to search for homogeneous social units begin with specific bounded units, such as census tracts, which are heterogeneous in nature, and which are usually fixed by relatively arbitrary boundary lines. They argue that the resulting distortion may be avoided by methods which "relate one social dimension to the territory at a time, and which relate this directly, without the intervention of bounded territorial units" (1971:66). The authors then demonstrate their procedure by drawing lines or isolines around homogeneous areas which enclose the ecological distribution of homogeneous social statuses within a community setting. Such a technique is consistent with the sociological concepts of community and subculture, but allows the boundaries of communities and subcultural areas to be drawn with a precision never before possible. The use of isoplethic mapping would guarantee a much greater homogeneity of the basic units of analysis in deterrence studies than can be achieved using such crude and relatively heterogeneous units as states, provinces, regions, and census tracts.[5]

Sample Contamination and Unknown Interaction

Unfortunately the isolation of social environments does not solve the problem of procedural spuriousness due to sample contamination and unknown interaction.

Sample contamination occurs in studies which use areas or populations in proximity. When comparisons are made among these areas, the results can be spurious due to "diffusion." Diffusion occurs when factors, thought to be affecting only some of the areas, are also affecting the other areas. For example, diffusion is the possible effect on the adjacent control states of the "Connecticut Speed Crackdown" (Campbell and Ross, 1969:46).

Unfortunately, it is difficult to isolate social environments which are free from outside influence, and which are therefore readily amenable to causal analysis (Przeworski and Tenue, 1970:25-26). Causal demonstration is often impeded because in most cases the social environment

> . . .is not effectively isolated, so that large numbers of outside influences are likely to be operating. Not only is it difficult to rule out many of the variables through randomization, but the observer also lacks adequate information about the temporal sequence involved. (Blalock, 1961: 3-4)

Unknown interaction results in spurious findings because of the inability to control for the interaction among the forms and dimensions of legal response within any one judicial system. Nettler points out that, despite the tendency of social scientists to assume few causes, social environments are probably comprised of many causes, interacting in non-uniform ways (1970:47).

Students of deterrence are not in agreement about the interaction of certainty, swiftness, and severity. Some researchers claim that the relationship of these factors is additive and linear (Gibbs, 1968a:523; Gray and Martin, 1969: 392; Bean and Cushing, 1971:289; Bailey and Smith, 1972:539). They maintain that increases in any one of the legal responses lead to a decrease in the crime rate.

Others claim that the responses interact and are curvilinear. Jeffrey suggests that increases in severity sometimes result in reductions in certainty (1965:299). Thus the President's Commission of Law Enforcement claims that the existence of capital punishment may result in juries being more likely to acquit an offender (1967b:27). Conversely, plea bargaining may result in increased certainty and decreased severity (Enker, 1967). Thus Logan (1972:69) suggests a curvilinear relationship between certainty and severity, where severity decreases the crime rate only when certainty is either high or low.

In the social environments of criminal justice systems, there is a strong possibility of unknown and unknowable interaction. If certainty, swiftness, and severity of legal responses interact, and if there is interaction within each of the three systems of inculpation, adjudication, and implementation, then there are thirty-six possible sources of interaction for any one justice system. This pattern does not even consider the additional possibilities of these interactions further interacting with the crime rate.

ASSUMPTIONS ABOUT LEVELS OF ANALYSIS

Deterrence researchers have rarely attended to the implications of aggregate data and areal sampling units in making assumptions about the continuity of levels of analysis. Areal aggregate data in deterrence studies are often the consequence of reporting units whose numbers and areas are unknown. However, deterrence researchers have assumed continuity over these reporting units and the levels of analysis.

The continuity assumption views different levels of analysis as identical, so that statements about any level can be generalized to include all levels (Hanna, 1971:1-3). On the other hand, the discontinuity assumption presumes that each level of analysis is separate and different, and that generalizations to higher or lower levels are, therefore, misleading. Thus, smaller or lower units of analysis cannot simply be regarded as miniatures of larger ones. They may, and often do, differ.

These two contrasting assumptions lead to varying interpretations of data (for example, the ecological fallacy flows from discontinuity) and to different uses of data analysis (for example, survey research and statistical techniques assume continuity across levels of analysis).

While studies of deterrence have used aggregated data, they have not taken into account procedural spuriousness, nor have they considered the relationship between mode of aggregation and sociological variables. In addition, researchers of deterrence have not adequately considered the value of the statistical techniques they use in analyzing their data. The usefulness of statistical techniques for answering questions about general deterrence is the topic of our next chapter.

FOOTNOTES CHAPTER NINE

[1] For example, consider data presented in the 1971 Uniform Crime Reports. In 1971, there was an estimated total of 41,890 forcible rapes known to the police (1971:12). During the same year, an estimated total of 20,120 persons were arrested for that offense (1971:115). The Reports assert that the "solution rate" for forcible rapes in 1971 was 55 per cent (1971:14), but they do not indicate how that figure was arrived at. Amongst those arrested for rape, 70 per cent were prosecuted for this offense and of those prosecuted, 35 per cent were found guilty of the substantive offense, while 17 per cent were convicted of lesser offenses (1971:14). However, it is probable that some of the rapes which occurred in 1971 resulted in arrest or conviction in 1972 or later. Conversely, it is possible that some of the prosecutions or convictions that occurred in 1971 relate to rapes which occurred in 1970 or earlier.

[2] The possibility of "significant" correlations being artifacts due to measurement error is enhanced when two or more measures are of the same underlying variable. The combination of overlapping measures of the same variable increases the magnitude of the association, called the "enhancement of reliability" effect (Campbell and Clayton, 1961). Measurement errors create the impression that more variables are involved in the phenomenon than is actually the case.

[3] Units of analysis that have been used for researching deterrence have been: (1) the nation (Barber and Wilson, 1968; Teevan, 1972; Jayewardene, 1972), (2) the region (Bean and Cushing, 1971), and (3) the state (Schuessler, 1952; Sellin, 1967; Gibbs, 1969a; Tittle, 1969; Gray and Martin, 1969; Chiricos and Waldo, 1970; Sjoquist, 1970; Bailey, Gray, and Martin, 1971; Bean and Cushing, 1971; Logan, 1972; Ehrlich, 1972; 1973).

[4] Smaller units of analysis include: (1) the county (Orsagh, 1973), (2) the municipality (Sjoquist, 1970), (3) the city (Dann, 1925; Savitz, 1958; Schwartz, 1968; Phillips and Votery, 1972; Votery and Phillips, 1972; Orsagh, 1973), (4) the university (Cambliss, 1966; Bower and Salem, 1972), and (5) police precincts (Carr-Hill and Stern, 1973).

[5] Occasionally, as Morris (1958) points out, even relatively small units of analysis, such as neighborhoods, can be heterogeneous enough to produce significant distortions

in the data generated. However, despite these limitations, the census tract will probably remain the smallest unit of analysis which is economically and pragmatically useful for the identification of homogeneous areas (Turner, 1969; Wolfgang, Figlio and Sellin, 1972:47-52).

CHAPTER TEN

CAUSALITY AND STATISTICAL TECHNIQUES

The statistical techniques used by social scientists are of several types, but many share a goal of attempting to calculate the relationship among variables by manipulation of the data (Simon, 1957:37; Lazarsfeld, 1968; Rosenberg, 1968: 131; Rosenberg, 1968; Blalock, 1969:22-26; Heise, 1969; Land, 1969; Goldberger, 1970). Adoption and development of these techniques has been defended, in part, as compensation for the limitations of research designs in social science. These lauded techniques are effect-to-cause analysis emphasizing the "decomposition" of the dependent variable to determine the independent variables responsible for it. This ex post facto analysis constitutes the primary mode of analysis in sociology (Greenwood, 1945:135; Duncan, 1966:1; Borgatta, 1969:xi; Costner, 1971:x; Wallace, 1971:101). Further, these techniques have been quickly adopted by the sociologists (Nygreen, 1971). Finally, to date, they are the most ubiquitous approach to the problem of spuriousness (Meehl, 1971b:143).

THE CALCULATION OF SPURIOUSNESS

The interpretation of a correlation coefficient as spurious may be achieved by statistical techniques that calculated that the correlation between two variables is the result of their common relation to other variables (Hirschi and Selvin, 1967: 73; Stinchcombe, 1968:33). In these instances, the calculation of spuriousness results from observations of the consequences for a correlation, while the influence of other variables is statistically controlled. Some of these techniques consist of part,[1] partial,[2] multiple regression,[3] and path analysis.[4] The procedures are similar in that they algebraically assign values to the extraneous variables and, by removing these values, adjust the original correlation coefficient.

Deterrence Studies and Statistical Techniques

Statistical techniques were applauded by students of deterrence as a solution to the problem of spuriousness. Many other deterrence researchers greatly value the use of these techniques. Wellford, for example, considers them capable of solving the problems of deterrence hypotheses testing (1974:122). Tullock maintains that they have already answered the whole question of deterrence (1974:103). He asserts that these techniques can determine the causes of crime, assign them values, and then gauge the influence of legal responses on crime rates. Bedau and Currie, commenting on the use of multiple regression techniques for deterrence study, claim that one "will want to scrutinize developments in the use of this methodology rather closely, since it promises to be both rigorous and influential" (1973:20).

Deterrence researchers have used several of these techniques. For example, Tittle computes a measure of association between crime rates and legal responses by using contingency tables to control for some of the presumed causes of crime (1969:414). Logan (1972) attempts to discover the association through linear and logarithmic analysis, while he controls for possible reciprocal effects of certainty and crime by part correlation. Bowers and Salem (1972) attempt to determine whether there exists a relationship between "formal sanctions" and rates of deviant behaviour by controlling for the "community normative climate" under use of partial correlations. Most recent studies, while controlling for numerous variables, have sought the degree of association through multiple regression analysis (Bean and Cushing, 1971; Ehrlich, 1972; Votery and Phillips, 1972; Phillips and Votery, 1972; Carr-Hill and Stern, 1973; Ehrlich, 1973; Orsagh, 1973; Sjoquist, 1973).

Unfortunately, these studies must be considered inconclusive. In the first place, the use of statistical techniques cannot overcome inadequate data. For example, Erickson and Gibbs, commenting on the use of statistical techniques, assert that "no statistical technique can remedy basic deficiencies of data. . ." (1973:536). Since most studies of deterrence use inadequate official statistics as data, the use of "powerful" statistical techniques seems irrelevant.

In the second place, few researchers attend to the problems of statistical techniques and: (1) the presumed relationship between the manipulation of data and the manipulation of variables, (2) the problem of interpreting changes in the magnitude of correlation coefficients when variables are controlled statistically, (3) the issue of determining the causal order of variables, and (4) the calculation of causal power, that is, how much of a cause is required to produce a desired amount of the effect.

LOGICAL AND EMPIRICAL ISOMORPHISM

Statistical techniques are systems of logic, and their use in sociology rests upon the assumption that they are isomorphic with empirical variables. Thus Werts and Linn claim that any statistical procedure is useful only to the degree that the underlying mathematical model approximates reality (1971:431). This assumption begs the very question. We use mathematical models to ascertain "how reality works," on the premise that the models already approximate reality.

This assumption of isomorphism results in an equation of the computation of values by statistical methods with the manipulation and measurement of variables (Meehl, 1970:40). More specifically, there is a confusion of: (1) the statistical concepts of "controlling" and "influencing" with the manipulation of variables, (2) the statistical concepts of "weighted" or "adjusted" values with the measurement of variables, and (3) the statistical concepts of "estimation" or "prediction" with the forecasting of outcomes.

Statistical Control and Manipulation

The statistical terms "controlling for," "holding constant," or "elaborating" mean that an association is tested for spuriousness by removing the "influence" of other variables (Rosenberg, 1968:24). Unfortunately, these terms cannot be regarded as referring to the manipulation of variables. As Blalock points out, "we pretend that the control variables are actually held constant" (1960:330). In fact, the "influence" of variables and their "control" is not the consequence of the measurement of manipulated variables, but rather the result of algebraic computation.[5] This computation assumes that (1) the control variables are normally distributed in the population, (2) they all have the same standard deviation, (3) this standard deviation is small (Blalock, 1960:325; Meehl, 1970:399), (4) the control variables are stable over time (Cain and Watts, 1970:243),[6] and (5) that they are not correlated with the independent variable (Darlington, 1968:178).[7]

Meehl contends that, as a result of these assumptions, the "influence" of the control variable is simply an algebraic artifact--a fictional score for a fictional and idealized population (1970:401). Since these scores are generated algebraically, without manipulation of the variables, the statistical terms "control" and "influence" are not referring to variables but to values.

Weighted Scores and Non-Spuriousness

"Weighted," "adjusted," or "standardized" scores are supposedly the correlations between variables once the influence of other variables has been removed (Blalock, 1960:328-333). These correlations are then represented as non-spurious associations. Unfortunately, they are not measures of non-spurious association, but are again, primarily mathematical artifacts. These correlation coefficients are artificial, in part, because they are not computed by the prescribed procedures (Blalock, 1960:

330). For example, the procedure for the algebraic computation of a partial correlation calls for the manipulation of the data by the technique of the method of least-squares, while, in fact, partial correlations are computed algebraically from the products and differences among the zero-order correlations (Blalock, 1960:330; Meehl, 1970:397).[8] For example, a partial correlation is a calculation of the supposed amount of variation in a dependent variable "explained by" an independent variable after the influence of the extraneous variable has been controlled for. The partial correlation is obtained by the following procedures: (1) Regressing the dependent variable and the extraneous variable. This produces a score called a residual, which allegedly represents the amount of variation in the dependent variable left after the extraneous variable has explained all the variation it can. (2) Regressing the independent variable and the extraneous variable. This produces a residual score which represents the amount of variation in the independent variable left after that explained by the extraneous variable. (3) Regression the residual for the dependent variable on the residual for the independent variable. This is a standardized score for the independent variable, which represents the influence of the independent residual on the dependent residual (Blalock, 1960: 329-333). Notwithstanding the foregoing definition of the standardized procedure for obtaining partial correlations, partial correlations are not, in fact, obtained this way (Linn and Werts, 1969:308). They are obtained by algebraic computations based upon three-zero order correlations (Blalock, 1960: 330; Meehl, 1970:397). The partial correlation is calculated using the zero-order correlations between the dependent and the independent variable, and between the dependent and the extraneous variable.[9]

In deterrence research, there is some evidence that even the assumptions for the algebraic computations have not been met. The algebraic computation of correlations rests upon the assumption of a difference in magnitude between the zero-order correlations and their residuals. Erickson and Gibbs, in reviewing the studies of Gibbs (1968a), Gray and Martin (1969), and Bean and Cushing (1972), claim that the differences between the zero-order correlations and their corresponding partials is negligible, and conclude that these "powerful" statistical techniques are therefore inappropriate (1973:541). In addition, when path and multiple regression analyses are used, these problems are magnified, since these coefficients are computed, in turn, from partial correlations.[10]

Estimation and Prediction

The statistical concepts of "estimation" and "prediction" are confused with the idea of prediction as a forecast. Statistical techniques produce "estimates"--scores which are statistical "predictions" about other scores; these estimates are then treated as predictions about the real world (Brewer, Campbell, and Crano, 1970:3). For example, Blalock (1960:33) uses the least-squares method to "estimate" the relationship between the percentage of the population "Black" and "discrimination." He also "estimates" the relationship between percentage of the population "Black" and of the population "Urban." Blalock then states that, from these two "estimates," the effects of changes in the percentage of the population "Black" on "discrimination" and "urbanization" can be predicted (1960:331). He assumes that statistical "estimates" and "predictions" are equivalent to statements about the quantitative changes in the relationship between the independent and dependent variables (Blalock, 1960:331). In deterrence research this issue is illustrated by Gray and Martin (1969: 391) who use partial and multiple regression analysis to determine the degree to which homicide rates can be "estimated" from severity and certainty of legal responses. Examining these estimates for linear and curvilinear relationships, the authors conclude that the curvilinear association is preferable, partly because it makes more "sensible predictions" about homicide rates (Gray and Martin, 1969:395). These "sensible" predictions are based upon the fact that linear regressions, but not curvilinear regression, can produce negative values. The implication is that one cannot have a negative homicide rate. At best it could be zero. In any case, Gray and Martin (1969) confuse statistical estimation with predictions about empirical variables.

Meehl doubts that the statistical "laws" which generate "estimates" are nomologicals, that is, "laws" about human behaviour (1970:339). He contends that the "estimate" entails the dubious ceteris paribus assumption, the belief that a change in the control of the independent variable would result in a specific and known change in the dependent variable, while everything else remained the same. Further, since the estimates are computed from algebraic formulae and not from the least-squares analysis of the data, they are artifacts and not predictions about outcomes.

SPURIOUSNESS AND THE MAGNITUDE OF CORRELATIONS

When statistical techniques are used, it is common practice to test only high associations for spuriousness, since it is assumed that decreases in the magnitude of a correlation indicates its spuriousness (Blalock, 1961:20; Lazarsfeld, 1968: 629).

Meehl (1970) and Rosenberg (1973) reason differently. They point out that low-order correlations are usually disregarded by researchers. However, if an association were non-spurious and low due to "suppressor" or "distorter" variables, then controlling for these variables might result in an increase in the original association. Changes in the magnitude of correlation are not _ipso facto_ related to spuriousness, and the decision about the meaning of a correlation depends on factors other than the size of the correlations or techniques for the computation of non-spurious correlations.

SPURIOUSNESS AND THE ORDER OF VARIABLES

Statistical techniques do not provide information on the order of variables (Meehl, 1971a:81).[11] Blalock states that attempts to determine spuriousness may fail because the technique itself cannot indicate whether the control variables are antecedent or intervening (1960:175). He indicates that a spurious relationship, where Z is the variable that causes X and Y, can be confused empirically with the situation in which Z is an intervening variable between X and Y ($X \longrightarrow Z \longrightarrow Y$ as opposed to $Z \longleftarrow X \longrightarrow Y$). It should also be pointed out that even if the variable is considered intervening, the flow of causation, from X to Y or from Y to X, is undetermined.

Some techniques used for the purpose of determining nonspuriousness are not consistent. Werts and Linn (1971:430) have shown that the equations for the method of least-squares involve contradictory assumptions about the order of variables. The formula for computing the degree of association between a dependent variable and an independent variable, controls for variables assumed to be antecedent but mixes the causal order of the variables. When we compute the association between the dependent variable and the independent variable, we treat the control variables and the interaction effects as antecedent to the independent variable. However, as the computation shifts to the determination of the amount of the association between the dependent and independent variables due to the control variables, the formula moves the independent variable and the interaction effects to precede the control variables. Finally, as the computation involves the determination of the amount of the association that is due to the interaction of the independent and control variables, the formula places the independent and control variables antecedent to the interaction effects. As a result, the computation formula involves contradictory assumptions about the causal ordering of the variables.

While the statistical techniques algebraically mix the order of variables, the final measure is computed as if the independent variables were all acting simultaneously (Blalock, 1960:326) without change over time (Heise, 1970:11).

Thus, the order of variables must be decided on some basis other than statistical techniques. Time sequence must be known, additional data gathered, or the order based upon an a priori assumption. Statistical techniques can manipulate only that which has been "put in;" they cannot decide what to put in.

In the study of deterrence, legal responses are commonly placed on the causal side of the statistical equation. Certainty is thereby assumed to be a cause and not a consequence of crime rates. However, when a high crime rate reduces certainty as a result of over-taxed police and judicial resources, certainty may become a consequence. Severity is also commonly treated as an input variable, but at the same time there is evidence that high crime rates can lead to over-crowded prisons and increases in the use of parole. In these cases, there is an obvious reduction of severity as a result of crime (Logan, 1972:72). Further, celerity can also be a consequence of the crime rate. High crime rates commonly slow legal responses.

CAUSAL POWER AND STATISTICAL TECHNIQUES

In dealing with alleged causal variables, statistical techniques often ascribe power to what is only a description of or an accounting for variance. This practice is due to the issues just outlined and to the non sequiturs that lead from the assumption that the logical manipulation of values is isomorphic with the control of variables to the belief that non-spurious measures of association are indicators of causal power. Thus the literature confuses techniques, which are believed to discover causal factors, with the measurement of a cause's capacity to influence. For example, Gray and Martin use partial and multiple regression analysis to examine the relationship between certainty, severity, and homicide rates (1969:379). They suggest that the crime rate will double if either certainty or severity is halved, and that the crime rate will quadruple if both certainty and severity are halved. While these authors point out that their statements are tenable only if causality holds, justification for their statements is based upon their conclusion that a linear "estimate" of certainty and severity "explains" 22 per cent of the variance in homicide rates, and a non-linear "estimate" explains "nearly forty per cent" (Gray and Martin, 1969:394). From their assumed identification of causal variables, they have attributed causal power to the amount of variance explained.

In contrast, Nettler observes that

> . . . measures of association, be they calculated as linear or curvilinear correlations or bivariate or multivariate regression coefficients, tell us something about the way measures "go together" for a particular sample at a particular time. The "going

> together" cannot, however, be interpreted as coincident with causal power. The student who translates a correlation or a regression coefficient into "variance accounted for" or "reduction in error of estimation" cannot then proceed to the further conversion of "variance explained" into statements about the likely consequences for Y of changing X. (1974:5, #14)

Variance Explained and Causal Power

The statistical concept of "variance explained" is not a measure of, nor can it be translated as, the capacity of a cause to produce effects. Nettler's caveats are warranted since the troubles with this logic and methodology are many. One obstacle to interpreting "variance explained" as representing the "amount of causal power" is semantic--the consequence of the mesmeric powers of words. Thus terms, such as "depends upon," "due to," "attributable to," "accounts for," "affects," "influence," "produces," "changes," "predicts," "independent effects," and "is controlled," deflect the consumers of statistical analysis from the algebraic and logical meanings of these words. These terms invoke preconceived beliefs referring to empirical phenomena. For example, Blalock, within two pages, shifts from the statistical term "relating" to the causal word "changing," to the full causal vocabulary of "producing" (1960: 343-345).

Blalock states that if we have

> . . .a large number of independent variables, we can obtain an indication of their relative importance by relating the dependent variable to each independent variable in turn, always controlling for the remaining variables. (1960:343, emphasis mine)

On the same page, some seven lines later, he states that each

> . . .coefficient therefore represents the amount of change in Y that can be associated with a given change in one of the X's with the remaining independent variables held fixed. (emphasis mine)

Two pages later, Blalock asserts that the

> . . .beta weight. . .indicates how much change in the dependent variable is produced by standardized variables when the other are controlled. (1960: 345; his emphasis, my emphasis)

A second obstacle to the use of variance explained as a measure of causal power flows from the first. As has been shown, the computation of "variance explained" does not involve either the manipulation of the variables or the data. As a consequence the idea of "variance explained" is more of an artifact than a measure.

A third obstacle rests in the assumptions about the past history of the variables under study. If some of these have been previously manipulated, then they could explain more variance than variables which had not been manipulated in the past (Aigner, 1970:249).

Fourth, as Simon and Rescher contend, no amount of mathematical manipulation can provide information about the efficiency of one variable to produce another (1966:324). These authors show that, if the research design does not permit the ordering of the variables, then no statistical technique can demonstrate causality. They show that any set of values that satisfies an equation will satisfy any mathematically equivalent set of equations (Simon and Rescher, 1966:329). If these authors are correct, then it is pertinent to point out that, within any one equation, if one computes a correlation, and then shifts the values of the variable X to variable Y, and also exchanges the values of Y for X, the equation will produce the same correlation for both cases. If such conclusions are sound, then statistical techniques are impotent for determining either causal connection or non-spurious association.

Finally, even if causal power could be measured, there would be no way of knowing or measuring the stability of that power over time and place. The potency of a cause may not be accurately represented as a generalization based upon a single study. There is no reason to assume that a single measure of causal power is indicative of its stability.

In addition, statistical techniques fail to provide information about: (1) which variables have been manipulated, (2) whether a variable can be manipulated, (3) which variable(s) from a range can be manipulated if manipulation is possible, and (4) how much change is required to produce a desired effect. Finding answers to these questions is beyond the algebraic computation of statistical techniques.

NON-SPURIOUSNESS AND CAUSALITY

Despite the truism that correlation alone is not evidence of causality (Simon, 1957:33), social scientists, including deterrence researchers, work with the assumptions that true associations can be separated from spurious ones and that non-spurious associations constitute evidence for causality. Measures of association and spuriousness are commonly directed towards answering causal questions (Brodbeck, 1968a:577; Meehl, 1971b:144). Blalock, for example, maintains that, if an association persists after being tested for spuriousness, a cause model exists (1961:20). Lazarsfeld asserts that the relationship between variables can be called causal when the control of any antecedent variable results in the continued relationship between the original variables (1968:629). These beliefs persist, ignoring the crucial question posed by Simon: "Are there any operational means for distinguishing between true correlations, which do imply causation, and spurious correlations which do not?" (1957:33).

In summary, the following arguments caution against the practice of assigning causality to allegedly non-spurious correlations: (1) the methods for separating true from spurious associations are uncertain;[12] (2) non-spurious associations remain associations[13] and, while associations are necessary, they are not sufficient evidence for attributing causality; (3) the relationship between statistical control and causal influence is vague; (4) no statistical technique is capable of establishing causality without a priori knowledge of the order of variables (Simon and Rescher, 1966); (5) without the power to manipulate variables, the identification of causality is impaired (Meehl, 1971b:143); and (6) confusion continues in the identification of causal variables with their capacity to produce (Nettler, 1974b:5,#14).

ATTRIBUTED SPURIOUSNESS

The weakness of research designs and statistical techniques has left the issue of spuriousness problematic. As a consequence, social scientists often attribute spuriousness. In doing so, they simply use the word "spurious" to repudiate the findings of a study. For example, a critic may claim that "findings" are probably false because the researcher failed to take into consideration the influence of certain variables. The critic declares that the reported relationship would probably vanish if controls for such variables were instituted. Other objections may arise from conjectures that a relationship among variables is "more complex than those examined or reported by the investigator" (Hirschi and Selvin, 1967:145). In these cases, the critic has attributed spuriousness to the findings, and he conjectures about the causal importance of the variables he suggests are responsible for the association. Meehl terms these assumptions about causality "problems of the interpretation of correlations" (1971:146).

The practice of attributing spuriousness in deterrence research is linked to questions of criminogenesis and crime reduction. To ascertain the effects of deterrence, some researchers consider it necessary to hold etiological conditions constant, since theories of criminality maintain that extralegal conditions affect crime rates (Gibbs, 1968a:117).

However, debates over spuriousness do not deal only with the causes of crime. Morris and Zimring contend that any assessment of deterrence also requires controlling for the nonlegal ameliorative processes reducing crime (1969:146).

The problems in establishing such controls are immense, for while there may be some knowledge of the correlates of crime, the causes of crime and its amelioration remain elusive (Nettler, 1974:253).

Since controlling for the causes of crime and its amelioration is unrealistic, researchers revert to conjecture. Osborne, for example, questions Gibbs' (1968a) finding of a relationship between the certainty of sanctions and homicide rates (1968:159). Osborne speculates that it is more logical to assume that sociological factors other than deterrence are responsible for low rates of homicide in certain states. Gibbs (1968a) maintains that Osborne's conjecture does not refute his findings, since Osborne would have to show, rather than assume, the correlation of crime rates and supposed etiological factors.

The dilemma of the elimination of extraneous variables is simple to formulate, but difficult to resolve. It is clear that analysis and evaluation are obscured when the association between the independent and dependent variables is either exaggerated or minimized by relationships with other variables which may be relevant or irrelevant. In any case, the effects of these variables must be eliminated or disregarded. However, if we do not know either the causes of crime or its amelioration, controlling these factors becomes guesswork. Assertions about which variables should be held constant will be based upon assumptions. Our lack of knowledge and our dependence upon theories that are culturally approved conjectures make these presuppositions problematic (Nettler, 1970:161-171;

Meehl, 1971b:147).

In the face of these difficulties it seems probable that investigators will continue to seek non-spurious correlation as a sign of causation and that they will do so under the problematic *ceteris paribus* assumption. It has been the tenor of our discussion to demonstrate that such a methodology is not adequate as a test of the causal efficacy of legal responses to crime.

FOOTNOTES CHAPTER TEN

[1] A part correlation is the degree of association between two variables after the influence of a third variable has been removed from one of the original variables (Logan, 1972:68).

[2] A partial correlation is the degree of association between two variables when the influence of one or more other test variables is removed (Theodarson and Theodarson, 1969: 82; Blalock, 1960:329).

[3] A multiple correlation is the degree of association between a dependent variable and a combination of two or more independent variables. Multiple correlation provides an estimate of the combined influence of two or more independent variables on the dependent variable. A multiple correlation can be either a linear or curvilinear correlation (Theodarson and Theodarson, 1969:82).

[4] Recently, path analysis has been used extensively in the analysis of cross-sectional data for causal inferences (Blalock, 1964; Duncan, 1966; Land, 1969; Heise, 1969). However, this technique is inappropriate for the testing of the deterrent hypothesis, for it assumes a recursive system, that is, the dependent variable never reciprocally influences the independent variable (Alker, 1968:217). Path analysis also assumes that there is no interaction among the variables (Heise, 1970:10) and that there are no changes in the causal relationships over time (Heise, 1970:11). In addition, the technique cannot yet adequately handle problems associated with aggregate data (Hanna, 1971:109) and is not appropriate for use on indices and/or transformations of data (Wright, 1968). Deterrence researchers cannot legitimately make any of these assumptions.

[5] The use of correlations to test a hypothesis about the nature of relationships among variables assumes that these variables have been measured without errors (Brewer, Campbell, and Crano, 1970:1; Werts and Linn, 1971:431). Further, Werts and Linn, in discussing the causal assumptions of the equations for the method of least-squares, argue that there is no "provision for errors of measurement. . . .reciprocal causation, or for the possibility that two or more observed variables measure the same underlying causal factors" (1971:430).

[6] Cain and Watts point out that the use of such "standardized" regression weights as beta-coefficients is usually predicated on an assumption (rarely made explicit) that the sample standard deviations use for adjusting the regression coefficients indicate a relatively fixed range of variation for the several variables. There is, in other words, some notion of "normal" limits of variation which are related somehow to the variation actually found in the population (1970:243).

Thus Blalock notes that the computations proceed as if "the control variable has been divided into very small intervals and separate correlations computed within each of these categories" (1960:332). The computations thus bypass the actual measurement of the variables.

[7] Darlington notes that, in multiple regression analysis, there are five different measures of the independent variables "contribution to the variance" in the dependent variable (1968:166). If the independent variables are uncorrelated with each other, then these five measures are equivalent. If the independent variables are intercorrelated, then the five measures are no longer equivalent, and the term "accounted for by" becomes ambiguous. Similarly, Hirschi and Selvin note that beta-coefficients as measures of the relative importance of variables is a common practice in regression analysis. But this practice is incorrect, except in those cases where the independent variables are essentially uncorrelated with each other. "By themselves, the beta-coefficient measure the "direct" contribution of each independent variable to the dependent variable, but they do not take account of the "indirect" contributions that each independent variable makes through its correlations with the other independent variables" (Hirschi and Selvin, 1967:158).

[8] A zero-order correlation is simply the measure of association between two variables.

[9] This involves obtaining the product of the correlations between the dependent and the extraneous variable and between the independent and the extraneous variable. This product, when subtracted from the correlation between the dependent and the independent variable, becomes the numerator of the partial correlation formula. The denominator of this formula consists of the correlation between the dependent and the extraneous variable, squared and subtracted from the integer 1; the correlation of the independent and extraneous variable, squared and subtracted from the integer 1. The square root of each of these values is obtained, and then their product is computed. Finally, the numerator is divided by the denominator.

This formula is as follows:

$$r_{13.2} = \frac{r_{13} - (r_{12})(r_{23})}{(1 - r_{12}^{2})(1 - r_{23}^{2})}$$

[10] Multiple regression techniques produce weighted correlations, called beta coefficients (Darlington, 1968:167). However, these beta coefficients are not obtained by regression analysis, but are computed algebraically by obtaining the partial correlation and then dividing this result by the standard deviation of the dependent variable and multiplying by the standardized standard deviation of the independent variable (Blalock, 1960:345; Cain and Watts, 1970:235). Similarly, path coefficients are computed algebraically from partial and beta coefficients (Nygreen, 1971:34). In sum, the statistical artifacts, called partial correlations, provide the basis for computing multiple and path coefficients. It seems reasonable to assume that this process leads us further and further from measurement.

[11] Statistical techniques assume linearity and asymmetry of causal order, and simply that the dependent variable never reciprocally influences the independent variable (Alker, 1968: 217; Hannan, 1971:115). Where data are aggregated, as in studies of deterrence, this assumption is moot (Hirschi and Selvin, 1967:149; Hannan, 1971:107).

[12] The creator of path analysis states that "the method of path coefficients is not intended to accomplish the impossible task of deducing causal relations from the values of the correlation coefficients" (Wright, 1925:444). Duncan affirms that path analysis is designed to facilitate interpretation, not to discover causes (1966:1). Blalock claims that no causal argument can be empirically verified (1968:155). Finally, Goldberger points out that the causal models are evaluated only in terms of statistical inference (1970:100), and Lerner claims that statistical techniques are developed and employed at the expense of causal issues (1965:7).

[13] It is important to remember that a correlation, whether true and/or non-spurious, is still a correlation, and as such, it does not constitute evidence for causality.

CHAPTER ELEVEN

SOCIAL POLICY IMPLICATIONS

Our study leads to the conclusion that to date hypotheses about general deterrence for serious crimes have not been adequately tested, and moreover, that they are untestable. Thus, if we wish to take a rational approach to deterrence theory, we have no means of establishing what to do on the basis of research. Yet, social policy decisions will continue to be made, with or without information from social scientists.

The implications of the preceding analysis for the formulation of social policy are oriented toward two issues: first, the feasibility of rational social policies regarding general deterrence, and, second, justifications other than general deterrence as legal responses to crime.

DETERRENCE AND RATIONAL SOCIAL POLICY

Theorists of general deterrence have been proclaiming social policy implications since the time of Beccaria (1764) and Bentham (1811). Considerations of legal responses to crime in terms of effectiveness for crime reduction are based upon "the rational ethic."

The Rational Ethic

The belief that "science can save us" has been called the rational ethic (Nettler, 1970:195). It is allied with the concept of social progress. Reason, knowledge, and rational action are believed to be requirements, for at most, reconstructing a better world and, at least, the amelioration of specific social problems (Cousineau, 1967:116). Supposedly man can attain scientific knowledge and then use it to solve his problems. One version of the rational ethic rests upon the assumption that science can discover the basic and invariant "needs" of man, ascertain the means which will fulfil these needs, and therefore unify man. In this perspective, truth, utility, and morality become synonymous. Such a belief provided the basis for the origin of sociology (Bailey, 1958:33; Cousineau, 1967:4-8). It is currently a pervasive ethic in the discipline and it permeates the general deterrence literature.

Limitations of the Rational Ethic

The rational ethic is impaired by several fallacies. The first two logical mistakes are naturalistic fallacies: the beliefs that (1) what "ought" to be can be deduced from knowledge about what "is," and (2) that what "ought to be" should be preferred (Nettler, 1968:203, footnote 12). Both beliefs rest upon *a priori* decisions about what ought to be, and such decisions cannot be scientific (Rhees, 1947:317; Cousineau, 1967).

Second, the rational ethic confuses efficiency with morality. For example, Chein's (1947) assertion that "good" actions always achieve their purposes epitomizes this confusion. Similarly, Cantril claims that, if past experiences have shown that certain actions are effective in attaining a goal, then those actions are "right" (1949:371). A critic of these authors argues that their assertions blur the distinction between morally (good) action and correct (effective) action (Nettler, 1950b:115).

The third problem with the rational ethic is the belief that, because science can sometimes determine consequences, the morality of these consequences can be evaluated scientifically.

A related fallacy is the belief that, because science can sometimes determine consequences, the morality of the actions which produced the consequences can be scientifically evaluated. These beliefs are problematic since it has not yet been demonstrated that scientific methods are appropriate techniques for assessing the morality of ends, means, or consequences (Nettler, 1950b:117).

Rationality and Deterrence

While the "rational ethic" underlies most recommendations concerning deterrence, it is clear that rational social policies flowing from this ethic are not possible.[1] The criteria for rational policy formation action cannot be met.

The Criteria for Rational Social Policy

The term rationality can be defined in several ways (Cousineau, 1967:13-27). A useful definition derives from Pareto's analysis of rational action (Pareto, 1935:77; Cousineau, 1967: 27-31). For Pareto, rational action involves the pursuit of empirical goals, consciously entertained where the means to achieve these are the most efficacious ones in light of scientific knowledge (Cousineau, 1967:27-31). Rational action requires the transitivity of goals, scientific information for selection of the most efficient means, and the ability to predict outcomes accurately (Cousineau, 1967:22). (These criteria separate rational action from instrumental action. In the latter case means may be efficient, but they are chosen on some basis other than scientific knowledge.) Our perusal of the literature on deterrence shows that these criteria cannot be met in the formulation of a deterrent policy.

Rational social policy regarding deterrence is unlikely, in part, because of the criterion of transitivity. Transitivity is defined by Rose as "that characteristic of rationality or consistency of choice such that if a person or group prefers A to B and B to C, he or it also prefers A to C" (1964:143). While the aims of any one member of the public, the legislature, or the judicial system may be transitive, we cannot assume transitivity at the aggregate level (May, 1954; Rose, 1964:148). There are several objectives of judicial systems of which deterrence is only one, even though it is a common aim. Given a multiplicity of goals, such as retribution, specific deterrence, incapacitation, and rehabilitation, intransitivity and conflict are likely to occur. Thus Allen notes that

> . . .no social institution as complex as those involved in the administration of criminal justice serves a single function or purpose. Social institutions are multipurposed. Values and purposes are likely on occasion to prove inconsistent and to produce internal conflict and tension. A theoretical orientation that concern for only one or a limited number of purposes served by the

> institutions must inevitably prove partial and unsatisfactory. (1959:227)

Even if deterrence were the only goal, conflict remains over the "best" means to attain it. Certainty, severity, and swiftness are rarely held to be all equally involved in achieving deterrence, and their relative efficacy is debated. In addition, there may be conflicts among legal responses that affect the gravity of offenses as opposed to those that affect the frequency of crimes. Nettler observes that social engineers, including students of deterrence, who attempt to solve problems through rational social policies, overlook the reality of conflicts and thus hold a defective view of social institutions (1970:207).

Scientific information is a requirement for performing rational actions. This dissertation has shown that available evidence to date neither confirms nor refutes general deterrence hypotheses, and that such data are not likely to be forthcoming. Thus, decisions about deterrence cannot be rational.

Finally, predictability as a criterion for rational action about deterrence is unattainable. The causes of crime and its amelioration are unknown. While knowledge of causes is not necessarily related to predictability, knowledge of associations is a prerequisite (Nettler, 1970:115). Our study reveals that identification of legal responses as sequentially and non-spuriously associated with crime rates has not been achieved, nor is it likely to be achieved in the future. Further, with the exception of the work of Schwartz (1968), there have been no attempts to determine whether there are differences among legal responses which affect frequency versus gravity of crime. Even if there were evidence leading to the conclusion that legal responses are somehow *related* to changes in crime rates and/or the gravity of offenses, knowledge of *causal* power remains in question. In brief, *this means that we have no way of knowing how much of a change in which legal responses, for how long, will deter how many people, from what kinds of offenses, for how long*.

RATIONAL SOCIAL POLICY AND THE COSTS OF CRIME

In order to be rational about social policy formulation with regard to crime and deterrence, we need to know the costs of crime and of the proposed solutions. For example, what are the costs of various deterrent policies versus incapacitation or rehabilitation? A brief survey of the literature on the costs of crime reveals little adequate information about the operating costs of police or the judicial and prison system. Without a base line telling us the costs of the judicial system, we cannot then compare the relative costs of a deterrence-, versus incapacitation-, versus rehabilitation-oriented criminal justice system.[1]

The Economic Approach to Crime

Many social scientists have been advocates of an economic approach to crime and social policy, originally conceived by Becarria (1764) and Bentham (1811). Becker claims that the economic approach has lost favour in the interim (1968:209), and Zimring and Hawkins state that it has not only been ignored but deliberately rejected (1973:51). More recently, this approach has been revised (Becker, 1968; Tullock, 1969; Stigler, 1970; Landes, 1971).

One approach to the costs of crime concerns the attempt to ascertain costs *per se* (Becker, 1968:169; Tullock, 1969: 59). This concerns the costs absorbed by the criminal justice system to be an important end. The aim of the approach is to determine and reduce costs *per se* (Zimring and Hawkins, 1973: 57).

The second approach to rational social policy concerns the cost-effectiveness criterion. This approach consists of four schools: (1) those who attend to the relative cost-benefit of any one kind of sanction type; for example, differential recidivism rates may be compared to the costs of different lengths of time of incarceration (Wilkins, 1967:24; Zimring and Hawkins, 1973:56); (2) those who compare the relative cost-effectiveness to the justice system of different kinds of sanctions; for instance, imprisonment versus probation (Becker, 1968:172); (3) those who count the "costs" to the offender (Zimring and Hawkins, 1973:43); and (4) those who consider the cost of legal responses versus non-legal remedies. To date, no one appears to have advocated this mode of cost-benefit analysis.

The Costs of Crime

The economic approach to crime runs into two major problems: (1) we do not know what the concept of "costs" involves, and

(2) consequently, its measurement is problematic.

Much of the literature on the economics of crime assumes that "cost" is a unidimensional, calculable quantity. Firey discusses the assumption that various courses of action can be rank-ordered and compared on the basis of some single dimension of cost (1969:72). However, cost is more likely to be a multidimensional concept, so that ranking or comparability cannot take place.

Zimring and Hawkins, recognizing such a problem, wonder how many robberies are equal to how many rapes (1973:54). Further, these authors point out that types of costs differ between the system and the offender and that it is, therefore, unrealistic to apply a single measure of costs to both, the state and the offenders (Zimring and Hawkins, 1973:57).

While there are different kinds of calculable costs, some kinds are not amenable to computation. Thus justice, humanity, pain, and suffering are believed to be unmeasurable concepts (Zimring and Hawkins, 1973:53). Campton wonders how to gauge the costs of inconvenience, self-esteem, repute, and the community's feelings of fear or safety that enforcement activities may engender (1969:428).

A perusal of the literature reveals only scant information about the actual costs of crime and its control. Most of this literature concerns hypothetical costs and is primarily prescriptive. Thus Wilkins (1967) presents a discussion of some issues and concepts, while Martin (1965) directs attention to the many kinds of costs to be gauged. Martin and Bradley (1964) outline the aims of a cost study and some problems of research design and conceptualization. More recently, economists recommend the economic approach to crime, and provide great detail on the mathematical formulae to calculate such cost (Becker, 1968; Tullock, 1967; Landes, 1971). While these computational tools are developed, there are few data available for such analysis (Tullock, 1969:71). Exceptions to this are the works of Martin and Gail (1967), who compute the total and per-police-officer costs of police departments for 12 boroughs, and The President's Commission, which calculates some expenses for imprisonment-per-inmate (Becker, 1968). In fact, the actual costs of the administration of justice for any one state, province, or city, or for any one time period are unknown. Thus we do not know the relative costs of any one sanction type, nor do we have comparative estimates of sanction costs, such as regards time and place.

Consequently, criteria on the cost of crime are debatable. Things which are counted today may soon begin to "count" (that is, create concern), and decisions as to what should be counted are a matter of preferences. In fact, the measurement of cost entails costs which, in themselves, may be disputed. In addition, the usefulness of data on the economics of crime may be evaluated in terms of their costs of compilation (Wilkins, 1967:24). Commenting on the costs of social policies, Nettler observes that our choices are often made without knowing what the costs will be, and that when and if these are ascertained, they are often higher than expected (1973:32). In addition, the results of the program are often "tawdry."

In sum, none of the criteria for rational social action regarding general deterrence can be met. This means that, for general deterrence, "there is no solution in the sense of there being a correct course of action likely to produce known and wanted results at a known and acceptable price" (Nettler, 1976:ii). It seems reasonable to consider the problem in different terms. One can, of course, proceed by making assumptions about deterrence and hope for successful social action. Alternatively, the pragmatic approach to deterrence may be abandoned and other options contemplated.

ALTERNATIVE RESPONSES

Lacking scientific knowledge about general deterrence, social policy formulators can take at least three stances. The first assumes that deterrence occurs and recommendations are made accordingly. Packer argues that our ignorance about the causes and modifiers of behaviour leads to the "inevitable" use of "punishment" (1968:249). Similarly Van den Haag contends that, if there is no evidence for or against deterrence, then there is a "moral obligation" to execute offenders at the risk of ineffectiveness (1969:147). The second stance assumes that deterrence does not occur and that recommendations are made accordingly. Thus, Sellin (1967) claims that, because there is no evidence for deterrence, capital punishment should be abandoned. Both of these positions appeal to assumed consequences. On the other hand, the third attitude regards legal responses as expressive or symbolic and attends primarily to their morality. This approach to legal responses does not consider consequences other than the symbolic.

Deterrence and Morality

Moral prescriptions are "categorical obligations," and are characterized by their "superiority" and "authority." Their "superiority" derives from their autonomy. They are deemed sufficient and while they receive justifications, they are not amenable to change when their justifications are challenged. In addition, moral prescriptions have "authority," which means that they have priority over non-moral prescriptions (Ladd, 1957:104).

Non-moral prescriptions are "hypothetical obligations" and concern the evaluation of goals in terms of their consequences. In this sense, they are instrumental and take the form of: "If you wish some end, then you should do the following" (Ladd, 1957:102).

These definitions of moral and non-moral prescriptions permit the treatment of moral prescriptions as autonomous, contingent upon the degree to which facts and values are independent. This independence is contested, but based on good reasons. One reason is rooted in the definition of both terms.[3] A second reason derives from data which shows that the distinction is made by moralists (Ladd, 1957:101-107). In addition, the utility of treating facts as distinct from values is supported by the inability of those who contend otherwise to maintain their arguments consistently.[4] Finally, supporting evidence for the independence of values is derived from studies which find that information seldom alters values (Nettler, 1973:9).[5] The autonomy of morality means that changes in morals need not be mapped against factual or technological changes, but can be regarded as changes within the morality itself (Nettler, 1972:371).

If some moralities are autonomous, then it is reasonable to suggest that some legal responses to crime are expressive or symbolic. Expressive legal responses are those which result in the satisfaction of an emotion (Davis and Sevens, 1975:6).[6] Symbolic legal responses function to persuade by organizing feelings, attitudes, and perceptions. They reaffirm belief in the defended morality, and designate superior and inferior moralities (Gusfield, 1963:170).[7] These approaches to legal responses have been suggested by several sociologists (Durkheim, 1895; Mead, 1918; Davis and Stevens, 1975).

SUMMARY

Generally, social scientists believe that social difficulties are like puzzles that can be solved rationally (Cousineau, 1967). This attitude converts social troubles to "social problems" which are believed amenable to rational solutions (Nettler, 1976:i). Such an approach ignores the possibility that the major concerns which confront human beings involve conflicting moralities, which are not soluble in the empirical analysis of consequences. A more adequate approach to some of our difficulties in living then, may be to consider some of them as "social concerns," those universal difficulties "objects of human hope and fear" which interest and trouble people but which are not convertible into manageable and solvable problems (Nettler, 1976:ii).

A conclusion of our inquiry is that quarrels about general deterrence cannot fruitfully be considered debates about a "solution" to the "problem" of crime, but rather, expressions of social concern.

FOOTNOTES CHAPTER ELEVEN

[1] Social intervention entails the concept of causality in at least two senses. The first notion is found in the assumption that causes must be known before planned action can ameliorate or promote the resultant conditions. This assumption is moot as "no one knows to what extent knowledge of the causes of human behaviour promotes rational action in response to it" (Nettler, 1970:145). The second notion of causality is imbedded in the concept of social intervention itself. In this case social policies are considered to have consequences, which are interpreted as having resulted from the programs.

[2] I am aware that this statement is endangered by tautological reasoning. In that Ladd defines morality in terms of its autonomy, it may be circular to describe moralities as autonomous. However, the extent to which facts and values are independent provides us with a way of ascertaining circularity.

[3] Facts may be defined as hypotheses about "phenomena verifiable by reference to publicly replicable and communicable experience." They are sentences which are declarative and predicate empirical phenomena amenable to scrutiny by public observation (Nettler, 1972:87).

[4] For example, Gouldner blurs the distinction and speaks of the "myth of a value-free sociology," but when challenged as to why one should attend to his thesis rather than to those of others, he justifies his theory in terms of "technical competence" (1962:37).

[5] On histories of science Nettler states that these

> . . .demonstrate that the factual evidence for the hypothesis is frequently not sufficient to compel its acceptance. A powerful factor affecting our belief is what we want, particularly when those desires are deemed to be those ultimately called "moral." (1974:1,#6)

[6] In short, the infliction of punishment by

> . . .law gives definite expression and a solemn ratification and justification to the hatred which

> is excited by the commission of the offense. . . .The criminal law thus proceeds upon the principle that it is morally right to hate criminals. . . .I think that whatever effect the administration of criminal justice has in preventing the commission of crimes is due as much to this circumstance (expression of hatred) as to any definite fear entertained. (Stephens, 1883:80)

Similarly, Cohen claims that

> . . .the sentiment of just vengance or retribution is too deeply grounded in human nature, and embodied in too many moral and religious codes, to be. . .lightly dismissed. (1950:42)

[7] "Exercise of the criminal law reaffirms what we are for and what we are against" (Nettler, 1974:33).

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